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XLVI.

A MANUAL OF COMPARATIVE ANATOMY;

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2. *Report upon the State of the Hospitals of the British Army in the Crimea and Scutari, together with an Appendix.* Presented to Parliament by Command of her Majesty. 1855.—pp. 357, folio.

THE history of the origin and progress of the war now waging in the East—the dark tale of incompetency and departmental mismanagement—the resulting misery and suffering among our troops—their brilliant deeds of daring in the battle-field—their indomitable perseverance in the siege operations, under privations of no ordinary kind—their discipline, their fortitude, and their uncomplaining patience under suffering, are events of too recent occurrence to require to be here recorded. The heroic exploits of the gallant army will be chronicled, as they well deserve to be, by abler pens than ours; and the fatal mistakes which have been among the chief causes of its sufferings, will, we trust, lead to a satisfactory reform in the military system of this country, which, by putting the right men in the right places, may prevent a recurrence of such deplorable events.

These are subjects which it would be out of place to discuss in the pages of a medical journal, but for the circumstance that much blame has been thrown upon the medical department of the army. Its organization has been loudly condemned by the newspaper press; its system of management has been called in question; an outcry has been raised against the Director-General, and he has been held up to public odium, as having, through his neglect, sacrificed thousands of our gallant soldiers. We propose to examine, by the evidence contained in the

Reports laid before Parliament, how far these charges can in justice be sustained; to whom blame can fairly be attached; to what extent the mismanagement is attributable to the system or to individuals; and what appear the most likely means to render the department more effective in future. Before entering upon these questions, however, it may be advisable to give a very brief and general sketch of the movements of the army, and the state of health at the different places where it was stationed.

In February, 1854, the government having decided to send an army of observation to the Mediterranean, to be ready in case of a declaration of war with Russia, 10,000 men were assembled at Malta. It was shortly thereafter resolved to increase the force to 25,000, and to send it to some place nearer the probable scene of its future operations. Accordingly, towards the end of April, the additional troops from England, as well as those already assembled at Malta, were landed at Gallipoli, on the shores of the Dardanelles, and subsequently moved to Scutari and Koolalce, near Constantinople. In June, the army embarked at Scutari and proceeded to Varna, in the neighbourhood of which it encamped. After a short time the force was spread over the country; the heavy cavalry and the third division remained in the immediate neighbourhood of the town; the first division encamped at Aladyn, a distance of nine miles from it; the second division about eighteen miles up the country; the light division at Devno, four or five miles further on, while the horse artillery and light cavalry were pushed forward as far as Jenibazar, a distance of forty miles from Varna. At Malta, Gallipoli, and Scutari the troops enjoyed good health; but about a month after landing at Varna, cholera broke out among the Sappers and Miners, and afterwards prevailed to a considerable extent in all the divisions of the army. Fever and diarrhoea were also very prevalent. It may be necessary here to observe, that in the selection of the ground on which to encamp the various divisions, the medical officers do not appear to have been consulted—the spot having been fixed upon by the general commanding the division. Indeed, General Bentinck states, that it is not usual to take the opinion of any medical officer on this point, “unless something occurs to render it necessary.” When the cholera broke out in the first division at Aladyn, the troops were encamped not far from a lake, from which very dense mists rose in the morning and evening,* and which was stated by the inhabitants to be a very unhealthy locality.† Shortly after the disease appeared, the division was moved by the Duke of Cambridge to Gevreckler, “about six miles distant, on the top of the hills, a long way from the lake.” General Bentinck states, that after the change the health of the men improved slightly, but the cholera and typhus did not quite cease; but the Duke of Cambridge does not admit that the removal had any effect upon the sickness. There can be no doubt, however, that the first site was badly selected, and that had the troops been from the commencement at Gevreckler, they would have had a better chance of escaping disease. This is but one of many instances illustrative of the necessity for a council of health with every army in the field, to advise the General Commanding on all sanitary questions, and which should have a powerful voice in the selection of the ground for

* Lieut.-Col. Wilson's evidence.

† Major-General Bentinck.

encamping, whenever strategic considerations do not interfere. The brigade of Guards lost from cholera and fever 170 men out of a strength of about 3000; the second division lost only about 60 out of 6200; while the light cavalry and horse artillery enjoyed a still greater exemption.

During the time the army was in Bulgaria, it was indifferently well-provisioned. Occasionally the supply was not very regular, owing to the want of sufficient transport, and the forage for the horses was often very irregular. Even at this early period of the campaign, the deficiencies of the medical department began to show themselves. When the various divisions were, as already stated, moved up the country, a general hospital was established at Varna, with a view to relieve the field hospitals of such cases as, from the nature of the diseases, were likely to remain long under treatment; but from want of transport for the sick, this arrangement was of little service. A few cases were sent down in native carts, but there was so much difficulty in procuring even these, and they were so unsuited for the transport of sick men, that it was carried out to a very limited extent. After some time, an ambulance corps, which had been organized in England, was sent to Varna, with proper waggons and the necessary equipments; but as, up to the date of the army embarking for the Crimea, the waggons were not supplied with horses,* it might have been left at home, so far, at least, as the removal of the sick was concerned. The supply of medicines and medical comforts also was lamentably deficient. When the light division was ordered to Aladyn, "it was with the greatest difficulty that the smallest allowance of transport was granted by the military authorities. No medicine-chests, reserve supplies of medicines, or comforts, were allowed to be carried, not even a small supply of medicine for the staff, &c.†" The ticking of a paillassé was issued for the use of the sick in the regimental hospital marquees; but when an application was made for straw to fill them, it was returned with the remark, that "the supply of straw for the sick was deemed unnecessary."‡

The difficulty of finding transport also prevented the necessary supplies of medicines and medical comforts being sent up to the divisions; and after the outbreak of cholera the want of these was severely felt. When at length the troops were again assembled at Varna, with the regimental panniers exhausted, and with the prospect of immediate active service, it was with the utmost difficulty that a very moderate supply of the most necessary medicines was obtained.§

In the end of August the army embarked at Varna for the Crimea, and, after various delays, effected a landing unopposed at the Old Fort, on the 14th of September. The night after the landing was very wet, and the men being without tents suffered a great deal in consequence. The following day the tents were landed, but, owing to the want of transport, they were again put on board ship before the army advanced on the morning of the 19th. On the 20th the battle of Alma was fought, and the army remained on the field of victory during the two following days, to remove the wounded to the ships, and to bury the dead. The

* Capt. Wrottesley's evidence.

‡ Col. Italy, 47th regt.

† Staff-Surgeon Alexander's evidence.

§ Staff-Surgeon Alexander; Lord Cardigan.

ambulances having been left behind at Varna, there were no means of conveying the wounded to the beach, but a very few arabas, and the stretchers carried by the bandsmen of the regiments. The navy, however, kindly lent their aid, and landed 1000 men with 500 stretchers, who rendered most efficient assistance, and the French ambulance corps gave their valuable services; so that by the 22nd all the wounded were embarked. On the morning of the 23rd the army advanced to the Katcha, the following day to the Belbeck, on the next to the Tchernaya, on the 26th arrived at the valley of Balaklava and occupied the town; on the following day the allied armies took up their position on the heights above Sebastopol.

The men had been landed in very light order, carrying only their great coats and a few things rolled up in their blankets, and leaving their knapsacks on board ship. As already stated, the tents had been re-shipped, owing to the want of transport, and each regiment was allowed carriage for only one bell tent, to be used as an hospital tent. The men were consequently exposed at night to the cold and dews, with no covering than their great coat and blanket. Diarrhœa and cholera, which had scarcely subsided, again broke out among them, and it is to be feared that many who were compelled to fall out on the line of march perished, whose lives might have been saved had an efficient ambulance corps accompanied the army. The tents were taken by sea to Balaklava, and were issued to the troops in the beginning of October, but until then the men were constantly exposed. About the same time the ambulance corps was brought from Varna, and was employed in carrying the sick and wounded from the front down to the general hospital established in Balaklava.

Two days after the arrival of the army before Sebastopol the siege operations were begun, and the trenches were opened. The work was very severe, the men being on picket or in the trenches usually three nights out of four;* and their health naturally suffered from it, diarrhœa and cholera becoming very prevalent. On the 25th of October the celebrated cavalry action at Balaklava was fought; and on the 5th of November the Russians attempted to carry the position of the allies, and were defeated in the bloody action of Inkermann, where their numbers put *hors de combat* exceeded the whole force of French and English engaged on that day. The wounded in these two actions were first taken to the field hospitals, and thence removed by the ambulances to Balaklava, where a large number were put on board ships, and sent down to the general hospital at Scutari. About this time the weather began to break, and a considerable increase took place in the amount of sickness, from the exposure of the men to wet in the trenches, the insufficiency of the tents as a protection against the weather, and the want of a change of clothing, their knapsacks not having been restored to them since they landed at Old Fort. But upon the 14th of November a terrific storm occurred, which blew down all the tents, converted the whole camp into a vast swamp, destroyed completely the road from Balaklava, and wrecked thirty-two transports laden with warm clothing, with provisions, medicines, medical comforts, and forage—in short, with almost

* Lieut.-Col. Wilson's evidence.

everything on which the comfort, the health, nay, the very existence, of the army depended. The consequences of this storm were most disastrous; the land transport, which had before been inadequate, completely broke down, the horses dying from over-work and want of food. The men were put upon salt rations, and these, though not often very deficient, were issued irregularly; the necessary fuel was wanting for cooking; the men, after long exposure to wet in the trenches, returned to their tents, where, overpowered by fatigue and sleep, they lay down in their wet clothes upon the ground, which had been trodden into mud; while the miserable bell tents kept out neither wet nor cold. As a natural result, dysentery in its most intractable form—scorbutic dysentery—became extremely prevalent. The ambulance horses having been made use of for general purposes by the quarter-master-general, succumbed, like the others, to the exposure and over-work, and there existed no means of removing the sick to Balaklava; they were consequently treated under canvas; and as many of the regiments were not provided with hospital marquees, the only shelter the unfortunate patients had, was the common bell tent.

The artillery lent their waggons when they could be spared, and the few remaining cavalry horses were employed in bringing down such of the sick as could ride—a most unsuitable conveyance for dysenteric patients. It was to our allies, the French, we were chiefly indebted for the removal of the sick, as they lent us their ambulance mules, with cacolets and litters. Even after warm clothing had reached Balaklava, it could not be brought up to the front, from the want of transport, and the men remained exposed to the wet and cold, most inadequately clothed, when there was an abundant supply of everything necessary within six or seven miles of them. The medicines essential to the treatment of the cases were not to be procured from the divisional medicine chests, which had been long exhausted; and when a medical officer sent, or, as sometimes happened, went himself to Balaklava, he frequently found that they were not to be had even there. Under such circumstances the surprise is, not that there was so much sickness, but that the whole army was not annihilated. Independent of the sick treated in camp and at Balaklava, there were admitted into the hospitals at Scutari, Abydos, and Smyrna, from the 7th of September, 1854, to the 18th of February, 1855, no less than 17,537 soldiers, being one-third of the whole of the army which left England, all reinforcements included, and of these upwards of 3000 died.

That this enormous amount of sickness was chiefly the result of over-work, exposure, inadequate clothing, and bad feeding, was shown by the fact that when means of transport were at length procured, by the formation of the Balaklava and Kadikoi railway, when abundance of warm clothing was provided, when fresh meat was again issued, when by a re-distribution of the siege works with our allies the men were a little relieved from excessive over-work, their health immediately began to improve, disease became less prevalent, and of a less formidable type; and the amelioration continued progressive until the army, before the end of April, was reported in a highly satisfactory sanitary condition. This view is further corroborated by the much smaller amount of sickness and

mortality among the horse artillery than the line. From not being employed in the trenches, the duty came round to the former only every fifth night, they were much better supplied with warm clothing and provisions, and had waterproof sheets to lie upon. The influence of these circumstances may, perhaps, be better estimated from the fact, that while at the end of January the effective strength of the army under Lord Raglan had been reduced by deaths and by sickness to less than half its original numbers, of 165 men of the horse artillery who were landed in the Crimea, there were at that date 140 fit for service.*

We shall now proceed to examine briefly what measures of preparation were adopted to meet the probable wants of the army in relation to the medical service. On the 10th of February, 1854, Dr. Smith, the Director-General, was instructed to provide all the necessaries for the medical department of an army of 10,000 men, about to proceed to Malta. So promptly was this order obeyed, that the whole of the stores were ready at the Tower by the 22nd. They were then put on board a sailing vessel, which was detained so long that the army had left Malta before it arrived there. The stores were then trans-shipped, and ultimately reached Constantinople on the 15th May, nearly three months after they had been reported ready for embarkation.

The steps taken by Dr. Smith to ascertain the nature of the climate, the diseases, and the salubrity of the country to which the army was about to proceed, were so judicious, that we shall give them in his own words.

“On the 13th of February, four days after I was informed that there was an army to go to Malta, with a great chance of proceeding to Turkey, I made a representation to the commander-in-chief, stating, that as we were ignorant of the nature of the country of Turkey, the nature of its climate, the nature of its diseases, and the nature of its resources, it was essential to the interests of that army that long before it should reach that country we should be in full possession of information on all those points. I proposed that three of the senior medical officers who were destined to serve in that army should at once be despatched; and I also proposed that they should be accompanied each by an engineer officer to make a military sketch of every place where it was possible that an army might have to halt. The application on behalf of sending medical officers was sanctioned, but not that for engineer officers to sketch the country. I immediately drew up instructions for the guidance of those medical officers, and they left this country about the 28th of February, about a fortnight after I first knew that the troops were going. . . . Dr. Dumbreck went to Vienna: he communicated with Dr. Sigmund, and remained with him, and saw everything at Vienna in the way of military arrangements; in about four or five days he proceeded down the Danube, accompanied by an interpreter found for him by the government, communicated with every post of the Turks, inquired into the character of the country, the diseases to which the people were subject, the supply of water, and the nature of the locality, whether healthy or unhealthy, and how far it would be advantageous or disadvantageous to an army requiring to encamp there. The two medical officers who went to Constantinople were Dr. Linton and Dr. Mitchell. I instructed Dr. Linton on his arrival there to proceed along the coast of the Black Sea, that being one of the military roads, to examine the whole of the country, and the diseases of that part of the country; then to proceed to the westward along the Balkan mountains, and down the principal road *viâ* Adrianople, making the same inquiries at every locality where it might be possible, from water

* Capt. Shakespear's evidence.

and other circumstances, that an army might halt. Then Dr. Mitchell, the third medical officer, I directed to proceed to Adrianople, at once from Constantinople, and make his examination to the south-west as far as Gallipoli, and further than Gallipoli, in fact." (Q. 8075-6.)

In about two months voluminous reports were received from these officers, giving full and detailed accounts in reference to every subject to which their attention had been drawn. A copy was sent to the army for the information of Lord Raglan and the principal medical officer, and extracts furnished to the authorities at home, where the suggestions were of such a nature as to require their assistance in carrying them out.

When the expeditionary force was increased to 25,000 men, there was some difficulty necessarily experienced in providing the requisite number of medical officers, but this was successfully met, partly by appointments of assistant-surgeons from the list of candidates, and partly by the employment on the staff in the East of the assistant-surgeons of the cavalry regiments and the depôts at home.

But in addition to these preparations, which were strictly within his jurisdiction, and were carried out under his own orders and superintendence, Dr. Smith brought under the consideration of the military authorities certain other measures likely to have an important influence on the health of the troops, but which the medical department had no power to carry out.

The first of these was the establishment of an ambulance corps,* to consist of 400† men, raised by volunteers from the depôts of regiments not going out to the East, and furnished with waggons for the carriage of the sick and wounded, and of the hospital tents and stores. The waggons were provided, and the corps raised; but instead of "able-bodied men, of good character, from Chatham, and efficient soldiers, it was decided that pensioners should be sent out," notwithstanding an energetic protest by Dr. Smith, founded on his experience of what pensioners are. Distrusting the utility of the men thus raised, Dr. Smith then proposed to organize an hospital corps of at least 800 Armenians, placed under regular military discipline, who should be furnished with stretchers or bearers to carry the wounded off the field, or where, from the badness or total want of roads, the ambulance waggons would not be available; and who might at other times be employed in the hospitals as orderlies.‡ After considerable delay and frequent pressing, the Duke of Newcastle at length consented to send out Dr. Brett, a retired surgeon of the E. I. Company's service, to form the corps; "but his mission was unsuccessful, he having been told on the spot the men would run away the moment they heard the firing; the corps was never raised."

The next matter brought under consideration was the clothing of the soldier. In a letter to the Military Secretary, dated 13th April, Dr. Smith recommended the adoption of a loose garment of lighter material in lieu of the red coat; the substitution of a light head-dress for the schako; the abolition of the stiff leathern stock; and the issue of flannel shirts and flannel drawers. In a subsequent letter, dated 28th April,

* Letter to Military Secretary, Feb. 18th, 1854.

† This was intended for an army of 10,000 men.

‡ Letter to Military Secretary, April 1th, 1854.

Dr. Smith brought to the notice of the Military Secretary the following recommendation in the report by Dr. Dumbreck, already alluded to:

“Of the severity of the climate during winter we have ample proofs, and on that account I beg most earnestly to recommend that timely measures be resorted to, with a view to protect the troops against the inclement weather to which they will be exposed should they have to pass the coming winter either in Turkey or in the Principalities.” (Q. 8771.)

These letters appear to have received due consideration from Lord Hardinge;* and although it was not deemed expedient to adopt all the recommendations, certain orders were given on the subject, and steps were immediately taken to provide the requisite *winter* clothing. A large quantity was delivered at the Tower by the contractors in the beginning of August, and application made by the Ordnance to the Admiralty for freight on the 7th of that month, and repeated on the 15th of September; but partly through the carelessness of the Ordnance in not marking their application “urgent,” and partly through culpable delay at the Admiralty, it did not leave this country till the 18th of October. It was sent out in the screw-steamer *Prince*, which took the 46th regiment from England to Balaklava, where she arrived on the 8th of November, and immediately landed the troops. Although the army had then begun to suffer from want of warm clothing, the supplies were not landed, but the vessel was ordered out of the harbour, and lay at anchor outside till the 14th, when she was lost in the hurricane, and in her were lost 53,000 woollen frocks, 17,000 pairs of woollen drawers, 35,700 pairs of woollen socks, 16,100 pairs of blankets, 3700 rugs, and 2500 watch-coats. Immediately on this being known at home, steps were taken to send out abundance of warm clothing of every description, but from the difficulty of obtaining steam transport, and the complete break-down which had taken place in the land transport, it was the middle of January before it began to reach the army, and considerably later before all the men were supplied—indeed, part of it arrived as late as the 14th of March! It is impossible to estimate the amount of suffering the soldiers must have undergone during the inclemency of the winter, or the extent of mortality which arose from this melancholy disaster. Well may Captain Milne say, “Had this ship not been lost, no inconvenience would have been felt in regard to warm clothing.” The blame, however, clearly rests with the Ordnance and Admiralty for having so long delayed its transmission, and not with the military or medical authorities.

Another measure of great importance recommended by Dr. Smith, in a letter dated the 11th of May, 1854, was to provide and properly fit up a number of ships for the sick and wounded of the army. These were to be used for three different purposes: some were to be employed to convey direct to England men never likely to become available for further service, or who were not likely to do so within a reasonable time; others were to be set apart for the transport of sick and wounded to hospitals which might be established at a distance from the army—such as those at Scutari, Smyrna, and Abydos; and a third set were to be convalescent establishments, for cases which were making no progress to recovery on shore. The last were suggested in consequence of the

* See Lord Hardinge's evidence.

benefits which had been derived in such cases at Hong Kong, and other places where malarious diseases were a source of great inefficiency. These recommendations appear to have been treated with neglect; the Secretary for War has no recollection of their being sent to him, and the Admiralty repudiates all knowledge of them,—in fact, beyond the Military Secretary, the Sebastopol Committee does not appear to have been able to trace them, and the only inference which can be drawn is that the letter was deposited in one of the pigeonholes in his office, and never again thought of. Had Dr. Smith's recommendations been adopted as to the first two classes of ships, all the opprobrium which has been thrown upon the medical department, in consequence of the sufferings of the unfortunate soldiers in what has not inaptly been termed "the middle passage," would have been avoided. The blame clearly rests with the authorities at the Horse-Guards, and not with the Director-General.

At a subsequent date, after reports had reached home of the disgraceful state of the transports in which the wounded had been sent to Scutari, Sir James Graham wrote a private letter to Admiral Dundas, "inviting" his consideration to the question of fitting one or two steam transports specially for this purpose. The invitation, however, appears to have produced no effect; an official letter was therefore sent, on the 28th of December, calling the admiral's attention to the subject, and two vessels were subsequently fitted up. It is but fair, however, to Admiral Dundas to state that he denies having ever received any orders from home to prepare transports for the sick and wounded—a matter which may well be left to be settled between him and Sir James Graham.

We have detailed these matters thus fully, because, in our opinion, they go far to exonerate the Director-General from the accusations of neglect and incompetence which were so freely brought against him; while, at the same time, they show a greater amount of foresight and consideration on his part than characterized most of the other departments.

The Duke of Newcastle and Lord Hardinge both find fault with Dr. Smith, as it appears to us unfairly, because he did not either reiterate his recommendations until they were attended to, or appeal to the Minister for War against the military authorities for not acceding to his wishes. But surely, when the head of the medical department had made his official recommendations, and transmitted his requisitions to the proper authorities, he transferred to them the responsibility of any subsequent neglect. His duties in superintending his own department were sufficiently onerous, without having superadded to them the task of overseer to the Commander-in-chief, the Board of Ordnance, and the Admiralty Board.

With an army in the field, the necessary arrangements for the treatment of the sick and wounded in field hospitals, and for the establishment of general hospitals in the rear, devolve upon the principal medical officer with the force. This appointment was, in the first instance, held by Deputy Inspector-General Burrell, who was succeeded at the end of May by Deputy Inspector-General Dumbreck, and he was relieved by Inspector-General Hall, who joined at Varna in the end of June. With the latter, therefore, rested the responsibility of all the arrangements at the date of invading the Crimea.

When the army was at Scutari an hospital was established there, and on the embarkation for Varna taking place, all the sick of the regiments were transferred to it. After disembarking at Varna, a general hospital was opened for the reception of the sick in case of the army taking the field, and, to form a depôt for medicines and purveyor's stores on which the medical officers in charge of divisions might draw for the necessary supplies. When the army embarked for the Crimea the hospital at Varna was broken up; part of the stores were put on board a ship to accompany the expedition, and the remainder were ordered to be sent without delay to Scutari, which was to be the general hospital for the sick and wounded of the army. Unfortunately, this order was not complied with—at first, from want of sea transport, and afterwards, apparently, from neglect on the part of the persons in charge of the transport department, and want of energy in the medical officer on the spot. Dr. Smith says:

“I may have written, and I have written, not condemning (the medical officers), but regretting that they did not show more positive determination with the authorities who had the power of doing what was necessary, than perhaps they did show; and communicating to those authorities, that if such a state of things was permitted to continue, it must be understood that all responsibility was removed from the medical department.” (Q. 8474.)

Much of the subsequent misery at Scutari arose from the hospital stores being still at Varna, where they were not required.

Before leaving Varna, Lord Raglan called upon Dr. Hall to furnish him with a statement of the amount of transport which would be required for the conveyance of the wounded, on the army taking the field. This he accordingly did, in a letter dated 3rd of August, in which he detailed at considerable length the whole of the arrangements he deemed requisite, and the various duties to be performed by the officers of the department. Dr. Hall recommended that with each division of the army there should be 2 large store waggons for head quarters, 2 smaller ones for brigades; 6 spring waggons, one for each regiment; and 96 canvas stretchers, with two bearers to each. These proportions would give 30 waggons for infantry, 6 for cavalry, and 6 for artillery; making a total of 42 waggons for the whole army; 288 stretchers, carried by 576 men, for the infantry; 24 by 48 men for the cavalry; and 24 by 48 men for the artillery—making a total of 336, with 672 bearers.

It will hardly be credited that after all “no waggons whatever were embarked, except three, and these had no horses, harness, or drivers.” It does not appear whether this was done by the quartermaster-general, under whom the ambulance corps was placed, or by Lord Raglan's orders, but he was of course responsible. Neither is there any evidence to show what steps Dr. Hall took upon the occasion, but if he did not energetically remonstrate with Lord Raglan, and protest against being thus deprived of all his *matériel*, he was guilty of a gross dereliction of duty. It would be quite as sensible to land artillery in the face of an enemy without their ammunition tumbrils, as to land the medical officers without their store waggons, or the means of conveying wounded men to the hospitals in the rear, or, as at Alma, to the beach for embarkation. The consequences of this unaccountable proceeding have been already detailed in these pages.

Dr. Hall likewise suggested that two steamers should be fitted up and

equipped as hospital ships, to receive the wounded if the landing of the army were opposed. The *Andes* and *Cambria* were told off for that purpose, but had merely the fittings of ordinary troop ships. Dr. Hall states they were not so large nor so well calculated for the service as he could have wished, and were wholly insufficient for the sick and wounded after the Alma. He was not consulted as to the vessels, or as to the number to be put on board of each.

After the army had reached the south side of Sebastopol and commenced the siege operations, a house in Balaklava was given over for a general hospital, and fitted up with the stores which had been brought from Varna. This hospital appears, by the evidence of Captain Dacres and Dr. Dumbreck, to have been got into a very creditable state; but on the 25th of October, when Balaklava was threatened by the Russians, orders were given, by an officer of the quartermaster-general's department, to dismantle it and embark all the stores, which was accordingly done. The hospital does not appear to have been reorganized efficiently, at least for a considerable time; it was chiefly used as an "entrepôt for men arriving late at night from the front, and who were embarked on the following day." In consequence of this state of the hospital, and of the total break-down of the land-transport, the field hospitals became in reality permanent hospitals. As already stated, only some of the regiments had marquees, the others being provided with nothing but bell tents, in every way unsuitable for the treatment of sick, whose sufferings were still further increased by the want of fuel. Under these circumstances it was deemed expedient, as often as transport could be procured, to send down the sick from the front to Balaklava, and embark them at once for the general hospital at Scutari. This leads naturally to the inquiry, What steps were taken to provide for their transmission? and as the sufferings of the unfortunate sick were very much increased from bad arrangements, it may not be out of place to go at some length into this part of the subject.

From the very commencement this important branch of the service seems to have been conducted in a very discreditable manner. After the army had landed at Old Fort, it was determined to send the sick down to Scutari, and for this duty the *Kangaroo* was told off. Captain Christie, the naval officer in charge of the transports, had a signal made to send all the sick on board, and neglected to make any limitation as to numbers. The consequence was every ship sent some, and the *Kangaroo* was overcrowded to such an extent that there was not moving space on the decks. A second steamer and a sailing vessel were then got ready, to which a large number of them were sent; but these were of course not fitted up for the reception of sick. Cholera prevailed on board, and 51 deaths are reported to have occurred, during the four days occupied in the transit to Scutari, out of about 1300 embarked.

After the battle of the Alma the wounded were brought down to the beach, as already stated, chiefly by the sailors and the French ambulance corps. There was only one vessel there which had been equipped as an hospital ship, the other having been previously despatched to Scutari. The *Andes* took about 450 officers and men, but as the number of sick and wounded brought down for embarkation amounted to upwards of 2800, the remainder, with the exception of 453, embarked in H.M.S. *Vulcan*,

were put on board transports in no way fitted for their reception. The medical officers of the navy were most active in superintending the embarkation of these men, and afforded much valuable assistance when they had been put on board. A serious miscalculation of the amount of transport likely to be required, appears to have been made by the authorities. Dr. Hall applied for two vessels only to be fitted up, and even if they had been of the largest class they could not have accommodated above 800 men. Admiral Dundas, also, in his evidence, says, "Who ever thought that there would be 1500 or 2000 men wounded? You could stow 300 or 400 men, with beds, on board those transports, but no one suspected that there were 1500 men wounded, and 1500 sick patients." This mistake, however, is scarcely excusable, for in a paper by Mr. Guthrie, giving a sketch of medical arrangements for a force of 12,000 men, 1500 is the number he computes as likely to be wounded in the first battle; and he adds his belief "that 24,000 men will probably suffer little more in one action, than half their numbers if opposed to a superior force." As a copy of this sketch was sent to the military and medical authorities, more ample provision ought clearly to have been made for anticipated casualties.

But if the unexpected amount of wounds and sickness be deemed sufficient to excuse the very defective arrangements after the battle of the Alma, the experience of that occasion ought most assuredly to have demonstrated the necessity for some more extensive and permanent provision, in case of another battle being fought, or sickness continuing to prevail so extensively in the force. But nothing of the kind seems to have been effected. After the army had reached Balaklava, and fatigue, exposure, and bad feeding had done their work in inducing disease, the sick were still embarked in vessels wholly unprovided with any of the essentials of hospital transports—without bedding, without proper means of cooking, without any conveniences beyond a few ship's pails, with a deficient supply of medical comforts, and with too few orderlies, and these often unfit for their duty. The consequence of this was a great amount of suffering among the unfortunate patients, and, it is to be feared, a considerable increase in the mortality, which by judicious arrangements might have been avoided.

It is difficult to say with whom the blame of this mismanagement rests. Sir James Graham's "invitation" to Admiral Dundas, to consider the propriety of fitting one or two steam transports specially for the use of the sick and wounded, does not appear to have received any attention. But Admiral Dundas repudiates all responsibility connected with the transports. "They were entirely under the management of Lord Raglan, Admiral Boxer, and Captain Christie." (20,495). This Admiral Boxer, who may therefore be considered responsible for the fitting of the ships, is thus described by Deputy Commissary-General Smith:

"Whatever I ask is regarded as a bore, or granted as a favour; and all the arrangements of one day, which have occupied great time, and caused me immense trouble, may be altered the next, as is frequently the case. Admiral Boxer, in short, is a confused man, and has but little control over the ship-masters, who entertain neither respect for, nor fear of him." (15,888.)

It is not to be wondered at that under such a superintendent the service was badly conducted.

But even when the transports arrived at Scutari, the miseries of the unfortunate soldiers did not end. They were, in many cases, detained on board several days, either till room could be made for them in hospital, or from want of boats in which to land them; on one occasion the sick were thus prevented landing for four days, in consequence of Admiral Boxer having applied the boats to another purpose.

The responsibility of an inadequate number of orderlies to attend upon the sick rests, not with the medical authorities, but with Lord Raglan. The Queen's Regulations fix the proportion of these at one for every ten sick—certainly not too high a proportion for such cases as were being sent from the Crimea to Scutari. But Lord Raglan, in a memorandum dated October 18th, with a view, doubtless, to diminish the drain of men from the already over-worked army, ordered the number to be sent on board transports to be in the proportion of one to twenty-five, "a number wholly inadequate for the service on which they were employed."

It does not appear from the evidence that Dr. Hall took any steps to make Lord Raglan acquainted with the disgraceful state of the transports, or to have them improved by the naval authorities. The medical officers in charge at Balaklava seem to have exerted themselves to the utmost of their means. Dr. Dumbreck states that there were two thousand five hundred empty paillasses which he attempted to get filled with hay for the use of the sick in the transports. "I applied for hay again and again; at last with difficulty I got 1000lbs., and I stuffed a hundred of them, and there the hay ceased." (11,508.)

The evidence of Captain Dacres, R.N., also shows that the neglect of the sick at Balaklava, which had been a subject of complaint in some of the newspapers, was not chargeable upon the medical officers, for he says:

"I never saw more attention paid to people (than to the sick and wounded coming down for embarkation). Their own people came down with them to Balaklava; their own soldiers brought them down, and there were surgeons on the wharf seeing every man put in, and there were people giving them drink and everything else, very different from what it had been after the Alma." (16,202.)

It has been already stated that when the army left Constantinople for Varna a general hospital was established at Scutari, to which the sick then under treatment were transferred. When the expedition to the Crimea was in contemplation, instructions were sent to prepare more extensive accommodation, and for this purpose a large barrack near the hospital was obtained. It was a building badly adapted for an hospital, but was accepted as being the only available one capable of affording sufficient accommodation. Some progress was made in cleaning and whitewashing it, but when the sick and wounded began to arrive it was still in a filthy condition, and the hospital stores, as before stated, not having been forwarded from Varna, it was deficient in the furniture and fittings requisite for the proper treatment of the patients. The general hospital, which was capable of accommodating from eight to nine hundred, was tolerably well furnished, but the barrack hospital was without beds or proper bedding, unprovided with the necessary utensils, having no operating-room, no dead-house, no wash-house, and a kitchen insufficient for the ordinary cooking of the establishment, and, of course, with-

out the slightest chance of preparing the extra diets and comforts, so requisite for the sick. Add to this that the purveyor was incompetent, the apothecary inefficient, and the commandant afraid to incur responsibility, and it will be admitted that it would have required administrative talents of a high order to bring the place into a satisfactory condition. On September 21st, when the sick first began to come down from the Crimea, there were only four surgeons and twelve assistant-surgeons at Scutari under the superintendence of Staff-Surgeon Menzies. On that date 1262 sick arrived, and were landed the following day; on the 24th there were 435 wounded brought down, and on the day following the same number; on the 26th there arrived 1104, on the 28th 317, and on the 29th 274; the last two batches being chiefly cases of cholera, severe dysentery, and diarrhoea. There were thus thrown upon these hospitals, ill-adapted for the purpose, badly provided with the necessary fittings, and having a very small staff of officers, no less than 3817 cases, chiefly of severe wounds or of formidable diseases, in the short space of nine days. Probably no establishment, however well organized, could have satisfactorily provided for the wants of such an influx of patients, and it is impossible not to sympathise with Dr. Menzies in the difficult position in which he was placed, and which he thus describes:

“ . . . My difficulties on the arrival of the wounded from the Crimea were wholly caused by Mr. Ward's [the purveyor] inefficiency, and not having made timely arrangements for supplying the required bedding, &c., for the sick and wounded in the Barrack hospital, although I urged him daily, again and again, at the same time he assuring me that I might rest satisfied that everything there would be in perfect order. Much of the difficulty experienced at this time also arose from our not being in possession of our stores, especially bedding, which was daily expected from Varna. . . . Notwithstanding numerous directions given both by Dr. Hall, the inspector-general, and myself, for their transmission, a very considerable delay took place from the want of transport; and it was not until I strongly urged Admiral Boxer to despatch a steamer for them that any decided steps were taken for sending these stores to Scutari; and when the first supply of boards and trestles arrived, they were useless without the bedding, which, by some extraordinary neglect on the part of one or other of the purveyors at Scutari or Varna, was not sent for some time after.” (Q. 9742.)

Dr. Menzies complains also of the want of steady non-commissioned officers and proper orderlies, and in another part of the Report he states, “I had the work of three deputy-inspectors on my hands when I gave up the charge.”

While Dr. Menzies appears to have worked indefatigably and conscientiously in endeavouring to discharge his duties, it is quite evident, from the whole tenor of his answers, that he was not possessed of those administrative talents which are essential to the efficient superintendence of such a vast establishment. Instead of organizing the officers under him, and allotting to each his proper duty, retaining to himself little more than the mere superintendence, he appears to have endeavoured to do more than the physical powers of any man could accomplish, and to have had no time or opportunity of seeing that his orders were carried out, and that his subordinates did their duty. He seems to have inferred that because he ordered a thing to be done, it was done. It is evident that he was not “the right man in the right place;” but that he was entrusted with duties for which he was constitutionally unqualified.

Taking this view, we are disposed to judge leniently of his failure, attaching blame not to him—for he could not, if willing, vacate the post—but to those who placed him there, and especially to Dr. Hall, who must have seen him to be unequal to the task, and yet did not replace him by a more efficient man. On one point, however, Dr. Menzies deserves censure. When applied to by Lord Stratford de Redcliffe to state if there were any wants at the hospital, and when offered an unlimited command of money to rectify these, if they existed, he reported that their wants were satisfactorily supplied, and that the sick and wounded had received every care and attention which their situation so imperatively demanded.* The only excuse for such a report is, that he too credulously believed the statements of the principal apothecary and purveyor—statements which were wholly erroneous, not, probably, from any intention to deceive, but from complete ignorance of the state of their stores, and utter incompetence to discharge their duties.

On the 3rd October, Dr. Hall arrived from the Crimea to examine into the state of the hospitals, and so far from removing Dr. Menzies, as being unequal to the duties, he reported to Dr. Smith, under date of 20th October, “that the whole hospital establishment here has now been put on a very creditable footing;” that “by the strenuous exertions and unceasing labours of first-class Staff-Surgeon Menzies, and the medical officers under him, all our difficulties have been in a great measure surmounted, and in a short time, I flatter myself, we shall have an hospital establishment that will bear a comparison with any ~~one~~ of the same magnitude formed under similar disadvantages, or indeed, I may almost venture to say, under any circumstances.”† With the evidence before us of the actual state of the barrack hospital at that time—the want of bedding for the patients—the total absence of anything like clean linen—the defective supply of the necessary hospital furniture—the filthy state of the wards, swarming with vermin—the wretched kitchen arrangements, by which the diets were most irregularly distributed, and the food often only half cooked—the want of proper orderlies—the disgraceful state of the latrines—the uncleansed condition of the corridors—the want of sufficient storage for the apothecary and purveyor—and the entire absence of anything approaching to arrangement or knowledge of the stores under their charge—with the evidence before us on these points, we are wholly unable to comprehend Dr. Hall’s Report, or to realize his ideas of an hospital on a “creditable footing.” Believing as we do, that he was honest and sincere in the expression of his opinion, there are but two modes of explaining it—either that, like Dr. Menzies, he took his information at second-hand, without verifying it; or that his notions on the subject of a creditable hospital are such as to prove him utterly unfit for the appointment of inspector-general.

On the 4th of November, Miss Nightingale arrived at Scutari, with the band of trained nurses, and in a short time a marked improvement took place, by the establishment of a kitchen for the extra diets, the distribution of clean linen, the formation of a wash-house and laundry, and a more general attention to the cleanliness of the wards. At the same

* Letter from Dr. Menzies to Lord Stratford. Appendix to Second Report of Committee, p. 698.

† Appendix to Third Report of Committee, p. 505.

time, there was a considerable increase of the staff, by the arrival of medical officers from England, and the duty was thus brought more within their physical powers.

On the 6th November, Mr. Macdonald arrived to distribute the fund collected in this country for the relief of the sick and wounded, known as "*The Times' Fund*," and was the means of providing a great many of those articles of diet, clothing, and hospital furniture which tended so greatly to the comfort of the sick, and which, from the disgraceful mismanagement of the purveyor's department, were not forthcoming from the public stores.

Notwithstanding these improvements, however, the mortality in the hospital continued to increase, and it was not till towards the end of January that it began to show a marked diminution. This may have been in some measure attributable to the greatly overcrowded state of the wards, which appears to have been unavoidable, as Lord Stratford de Redcliffe was unable to obtain additional buildings, and the sick continued to pour down in great numbers from the Crimea. But it was chiefly owing to the condition to which the army before Sebastopol was reduced by the hardships, privations, exposure, and over-fatigue, to which it was subjected, and which gave rise to the worst form of scorbutic dysentery and typhus fever. From the period when the warm clothing and huts began to reach the army, may be dated the amelioration in the diseases treated at Scutari.

As allusion has repeatedly been made to the defective state of the apothecary and purveying departments, it seems necessary to make a few remarks on their condition before concluding this summary. In 1830, from motives of economy, these two departments were abolished. The duty of purveyor devolved upon the surgeon; and that of apothecary, in any of the large garrisons in our colonies, was performed by a second-class staff-surgeon. Some time before the war broke out the purveyor's department had been re-established on a different footing, and on so limited a scale that in the end of 1853 there were only three purveyors on full pay. It became necessary, therefore, when the army was sent to the East, to increase that branch considerably, which was done, partly by calling up men from half-pay, in the hope that their experience might prove useful, and partly by new appointments. The principal purveyor "was a man upwards of seventy years of age, who was exhausted with a walk between the general and barrack hospitals (at Scutari), only about a quarter of a mile. In addition to his own physical infirmity, he had only two assistants, and, I think, three boys as clerks, who were expected to do all the purveying for the sick and wounded men."* This was the officer of whom Dr. Menzies complained so seriously, as being the cause of all his difficulties. Shortly after Mr. Ward, the officer in question, went out to the East, rumours reached Dr. Smith that he was not efficient. A Medical Board was in consequence assembled, to examine and report upon him: they found him inefficient, and he was ordered home. Lord Raglan, however, at the instigation of Lord de Ros, the quartermaster-general of the army in the East, remonstrated against this decision, and, in consequence, Mr. Ward's recall was cancelled, and he remained at the

* Mr. Macdonald's evidence.

head of his department at Scutari till his death in January, 1855. Not only, however, was Mr. Ward inefficient, but the whole of the arrangements were made with so parsimonious a hand, that the officers of that department had not enough of assistance to enable them to carry on their duties. Purveyor Jenner, who appears to have been an active and energetic officer, says:—"On leaving Varna I had not a clerk, serjeant, or even orderly. I had to break open my cases, issue the rations myself, and deal out all the medical comforts myself." In addition to these evils, there was for some time a difficulty with respect to the position of the purveyor, who conceived himself to be independent of the control of the principal medical officer, and amenable only to the orders of the Secretary at War. This was ultimately rectified by instructions sent out from the War Office, directing him "to obey such orders as the inspector-general of hospitals, or the principal medical officer in charge for the time being, may think necessary for the welfare of the sick and the interests of the public service."

The apothecary's department was equally inefficient. A staff-surgeon of the second class, who had formerly served as an apothecary to the forces, was sent to Scutari to take the principal charge; but he appears to have been quite unfit for it, and his assistants were all men of no practical experience. Mr. Maxwell, one of the commissioners sent out to Scutari to report upon the hospitals, says, that upon examining the apothecary's stores he asked to see the books, and was shown two a letter book, and one for entering the requisitions or the state of the stores in—"there had not been an entry made in either of them from September 24th to November 28th." "We examined the stores and the books, and we found both in such a state of confusion that we thought it impossible to obtain any trustworthy returns from such a source." (Q. 12,982.) The department seems to have been conducted in a hap-hazard manner, and the results were such as might have been anticipated. Requisitions from the Crimea for supplies of medicine were met by a very partial compliance—for example, on one occasion a demand for 50 lbs. of opium was answered by sending 5 lbs.—and it often happened that when a requisition for some particular medicine had been met by the answer, "none in store," a subsequent search produced abundance of it. The result of this was, not only that the sick at Scutari suffered, but at Balaklava, which ought to have been supplied from the general depôt, there was constantly a scarcity of even the most necessary medicines, and it was often impossible to furnish the medical officers in the camp with the supplies which they so earnestly demanded.

What was obviously required was some one who had been accustomed to take charge of large quantities of medicines, and who had acquired such habits of business as to enable him to superintend the issue of these, and to keep up the stores requisite to meet the probable demands upon him. A gentleman possessed of these qualifications was at length sent out as principal apothecary, but not until about the end of January.

In the preceding brief sketch we have endeavoured, honestly and impartially, to narrate the principal events in connexion with the Medical Department of the Army in the East, and to trace, so far as the evidence will permit, the shortcomings, defects, and mismanagement to their true

source. Much of the blame which has been laid upon the department appears justly to lie with other branches of the service over which the medical officers had no control—especially the transport service, both by sea and land. Some part of it is due to the neglect of those recommendations which Dr. Smith had made to the military authorities, but which were not attended to by them; and a portion is attributable to the defective organization of the medical department, consequent upon the abolition of the apothecaries and purveyors, from shortsighted motives of economy. We have already stated that Dr. Smith's evidence and correspondence go far to exonerate him from blame; but in two points he seems to have failed. When he found his recommendation on *vital points* neglected, he ought to have offered the Duke of Newcastle the alternative of carrying them out, or accepting his resignation. Censure might have been directed against him for quitting his post at a moment when such exertions were required of his department, but the blame would have attached to the Duke of Newcastle for adopting that alternative. Again, when Dr. Smith found that he had been misled by Dr. Hall's report of the state of the hospitals at Scutari, he should have recalled that officer; by neglecting to do so he virtually adopted his opinion, in opposition to the mass of evidence brought against it. We are, notwithstanding this, disposed to concur with Lord Hardinge in his estimate of Dr. Smith:—
 “I have a very high opinion of him; I think him a very honourable and conscientious man, and he performed his duties, so far as I have been able to form an opinion, with very great precision, and a great desire to do what was right; and he is a very faithful and good public servant.”
 (Q. 20,864.)

Throughout the whole mass of evidence in the Reports, there is none to show that Dr. Hall ever represented to Lord Raglan the inefficient state of the purveyor's and apothecary's departments, or the disgraceful condition of the transports for conveying the sick and wounded to Scutari. Nor does he appear to have protested against the total want of any proper conveyance for the sick and wounded when the army embarked for the Crimea. He seems to have been deficient in energy and firmness, accusing an officer, in one instance at least, of making difficulties, instead of endeavouring to supply him with those things he required as essentially necessary to the welfare of the division of which he was in charge.* Of Dr. Menzies, as head of the hospital at Scutari, we have already spoken.

Against this apparent apathy and inefficiency of the inspector-general, and want of administrative ability on the part of the principal medical officer at Scutari, it is pleasing to set off the testimony borne on all hands to the zeal and energy of the medical officers generally. Mr. Dundas, M.P., who was at Scutari in December, says, “With regard to the hospital at Scutari, I am disposed to give every credit to the medical men there; I believe that they spared no exertion to do what they could to put matters in order, only the work was too much for them.” And Mr. Sidney Herbert, in the House of Commons, observed, “Every account I get, says this, the medical men in their vocation are beyond all praise, especially those at the head of the establishment; they work night

* See Dr. Alexander's evidence, in the Report of the Commissioners.

and day, their tenderness to the sick, their humanity, their zeal, their energy, are mentioned by every one, friend and foe."

The duties of the medical officers in the camp were of the most arduous and harassing nature. Suffering in their own persons the same privations and exposure as the soldier, they had not only the whole of that enormous crowd of sick to treat that afterwards inundated the hospitals at Scutari, but also many thousands of cases in the tents, which terminated before the means of transport to Balaklava could be procured. Those only who have encountered such terrible visitations as the famine-fever in Ireland, or the worst outbreaks of cholera among the destitute poor, can justly appreciate the labour and anxieties these officers must have undergone in the camp, and, knowing this from experience, will doubtless judge leniently their shortcomings, while they award to them the credit due to their patience, their exertions, and their unflinching perseverance.

The length to which our remarks have already extended compels us to be very brief in our notice of the changes which seem necessary to insure, for the future, greater efficiency in the medical department on service. In the first place, the Director-General should be in direct communication with the Minister of War, and not, as heretofore, under the control of several departments, and attended to more from courtesy than as a matter of right. Dr. Smith, in his evidence, remarked with much truth, "All the wants arose from a divided authority, the medical department being a sort of parasite department, ~~living~~ *living* upon the rest." This is, therefore, the first and principal change required. Among the others may be mentioned, the organization of an efficient ambulance corps, for the removal of the wounded from the field of battle, and the conveyance of the sick on the line of march, or to the hospitals in the rear of the army. This might well be formed as a branch of the Land Transport Corps, but ought not to be available for any other employment at the wish of the quarter-master-general's, or any other department. The formation of a body of qualified hospital stewards and orderlies is, we understand, at present being-carried out; and, as the men will not be drilled as soldiers, there will be no chance of their being removed from the hospital, as was formerly the case, for military duty. The Apothecaries' Department must be re-organized, and put on an efficient and permanent footing; it should be supplied with officers, not from the list of assistant-surgeons, but from persons in civil life who have had practical experience as pharmaceutical chemists, and been used to the handling of drugs. The appointment of purveyor should be abolished, and the duties devolved upon the commissariat, by whom they could be much more easily and satisfactorily worked. A branch of that service might, if deemed advisable, be formed for the "hospital commissariat." By such an arrangement much trouble, annoyance, and labour would be saved, at the same time that economy would be introduced, and a great probability of efficiency secured. On one other point an important change is necessary,—a fair proportion of the rewards and honours of the army must be granted to the medical officers. While brevet and substantive rank has been showered upon the fortunate military staff, and while promotion for "distinguished service in the field" has been conferred on not a few

nothing has been done to reward the medical officers who have shared with them the dangers of the field, and encountered in a far greater degree the risk of the pestilence, which has cut off at least ten times as many as the enemy.

It is not reasonable to expect zeal and energy in a department which is forgotten in the distribution of honours, and whose only reward, and apparently deemed by the authorities a sufficient one, is the testimony of an approving conscience. The risk they run is shown by a return given in by Dr. Smith, from which it appears, that of 529 medical men, including apothecaries, dispensers, and dressers, who had gone out to Turkey, 29 died previous to the 7th March last—being $5\frac{1}{2}$ per cent. in less than eleven months. The mortality since that date has unfortunately been very high, chiefly from fever; but as we have no correct information of the numbers who have died, we have confined our remark to the period included in the official documents.

The history of the department, as brought out in this investigation, furnishes an instructive lesson on the folly of that economy which reduces national establishments far below the point of efficiency. The same parsimony which deprived the medical service of its necessary numbers and organization, had so reduced the military force itself, as to oblige the government to send out large drafts of newly-raised soldiers, who, not inured to fatigue, literally melted away under the hardships of a winter campaign, in such numbers that they not only encumbered the already crowded hospitals, but furnished a very large proportion of the whole mortality of the army. A false economy thus entailed as its consequences much misery and suffering among the sick and wounded, and a deplorable sacrifice of human life.

Perhaps we cannot conclude this article better than by quoting the eulogium passed upon the medical officers by the Duke of Newcastle, when moving the thanks of Parliament to the army and navy of England:

“I must state, in justice to an honourable profession, that never were greater exertions made by any body of men—never was more humanity evinced—never more complete devotedness to their duties, than by the medical officers of the British army in the Crimea. To one of these men I must allude. I will ask your Lordships to consider for one moment the services performed by such a man as Dr. Thomson. He was left, under circumstances of the most painful nature, upon the field of battle of the Alma, with not another person to assist him, not to attend to the wounded of his own army, all of whom had been removed, but to a large number of Russian wounded, many of whom, persuaded that an Englishman was little less than a devil, were prepared to murder any individual who might seek to render them succour and assistance. Among such men was Dr. Thomson left alone. He bound the wounds of some hundreds of these poor Russian soldiers, at the great danger of his life, but nevertheless he escaped. He returned to his duties in his own army; but it pleased Providence to remove him from his sphere of usefulness two or three days subsequently. His death was occasioned by the immense exertions he had made, and a disease which he had brought on by his extraordinary sacrifices and toils. I must say, my Lords, that if it has not been usual for Parliament to thank such men as these, at least it is not wrong for a Minister of the Crown to stand up in this House and express his admiration at such conduct.”*

REVIEW II.

1. *Pathological and Clinical Observations respecting Morbid Conditions of the Stomach.* By Dr. C. HANDFIELD JONES, M.B. Cantab., F.R.C.P., F.R.S., Assistant-Physician to St. Mary's Hospital.—London, 1855. pp. 226.
2. *Observations of Morbid Changes in the Mucous Membrane of the Stomach.* By Dr. HANFIELD* JONES. ('Medico-Chirurgical Transactions.' Second Series, Vol. xix. p. 87.) pp. 66.

THE stomach is an ill-used viscus. It has been flattered in metaphor and insulted in fact. The poetical prose of the orator Menenius Agrippa makes it a type of a beneficent aristocracy; and the prosaic verse of the physician Sereus Sammonicus crowns it a king.† Persius dubs it a Master of Arts—*magister artium ingenique largitor*—and Aretæus a general (*ηγερμῶν*) of joy and grief. In the early part of the present generation, popular parlance referred to it all classes of ailments, bodily and mental: while medical men pronounced the viscus itself a mere bag, and all special attention to it quackery; and the scientific authors of the 'Cyclopædia of Practical Medicine' added the final insult to injury, by finding no place, save in a Supplement, for "Diseases of the Stomach and Pylorus." It will probably not be questioned, that there is no part of the body of which we hear so much from our patients, and are able to communicate so little real knowledge in return, as about the stomach. Its physiology has indeed been a good deal attended to, but its pathology has as yet had scarce a ray of light thrown upon it by modern science.

The publications named at the head of this article are the products of an attempt to clear away some of the mists from that dark region of pathology, and cannot, therefore, but strike the reader as worthy of great attention. Possibly he may expect to find in them more than the present state of science justifies, and then he will be disappointed; but, at the same time, he must acknowledge that the author is on the right road to truth, and is sure, therefore, to arrive at his goal before those who attempt the short cuts of theory and imitation—*claudus in viâ antevertit cursorem extra viam.*

The reason that induces us specially to pronounce the author "on the rails" to a correct knowledge of the morbid processes of the stomach, is, that he puts himself under the guidance of the two best engines of pathological research, as yet unapplied to this subject. The first is, *the exami-*

* This spelling of Dr. Handfield Jones's name is the first in a series of 81 misprints to be found in the 66 pages of the paper. Some are of considerable importance, such as, "swollen," for "smaller;" "anterior," for "cut;" "valve," for "ventricle;" "less," for "more or less;" "appearance," for "appetite"—some ridiculous, such as describing an aged female as a "foot-man;" and "5 crepitations" in the lungs. For these the Society render themselves responsible, by not sending proofs to the authors for correction. It is to be hoped that the rule may be altered before the next annual issue, or that at least each volume may contain a list of the errata in the last.

• † *Qui stomachum regem totius corporis esse contendunt, nisi verâ ratione videntur.*

nation and record of the appearances of the same part in a consecutive series of unselected cases; and the second, the skilled use of the microscope.

The method of taking a consecutive series of cases is the only way to place a guard over that weakness of the human mind which causes positive events to take a much firmer hold on the memory than negative ones, which allows the seeing a thing a few times to outweigh the hundred times we do not see it under similar circumstances, and consequently allows false inferences to be drawn. The only way to avoid this is to take equal note of each case for a definite period or number, and to reckon up our statistics at the end of the series. We shall thus get a notion of the real frequency or rarity of the phenomena we are observing, and of their consequent importance, scanty perhaps, but ten times the worth of a collection of facts made sporadically, or with the view of proving a predetermined point.

The application of microscopic inquiry to such a series of consecutive cases is almost unique in any department of morbid anatomy. Indeed the labour involved is so great, that it scarcely surprises us to see even those best qualified by knowledge and leisure shrink from it. Certainly in the observation of the stomach nothing has been attempted at all like the task which the author of the works before us has performed. Professors Engel and Wedl have cursorily noticed that the glands of the stomach are sometimes degenerated; Professor Rokitansky has didactically suggested some differences in the forms of ulceration, &c.; but any notion of how frequent and how rare these lesions are, or with what other morbid states they are conjoined, is now first attempted to be given. It is not, of course, pretended that the one hundred cases here analysed put us in possession of the full information which is required as to the relation to one another and the comparative frequency of these lesions; but they show, at any rate, some of their bearings, and their very great frequency, and they are a guarantee that the threshold is passed of a most important inquiry, and one in which the aids of modern science may be most advantageously employed.

Without a microscope, very little can be done in this part of the body. The surface of a mucous membrane, unless absolutely destroyed by solution of continuity, exhibits scarce any evidence of chronic changes—certainly no evidence which is not easily masked by acute, temporary, or even *post-mortem* appearances. In painters' phrase, the accidental quite overpowers the local colouring. Dr. Jones truly remarks:

“If any one should desire a proof of the great advances made in exact information by the aid of the microscope, he may find it in comparing the account given by Haller of this tissue, scarcely a hundred years ago, with our present knowledge. In his view it was a continuation of the epidermis, and, like it, might be thrown off and regenerated. Like the epidermis, too, its principal duty was to cover and protect the subjacent membrane, the ‘*tunica nervea*,’ *ne perpetuo doleat*.” (p. 13.)

Now, however, we know that this supposed homogeneous coat, this mere defence against accident, contains (or rather consists of) an infinite multitude of actively secreting glands, so numerous and close that there is hardly room for anything else besides them and the bloodvessels between them, yet each as distinct from the others as the separate fingers of a glove.

It is curious how few people have examined these glands, yet it is so

interesting, and gives such a striking picture of the minute activity of nature, that it is worth the while of any one who can obtain access to a microscope to examine them, however busy he may be. There is no particular difficulty in obtaining this view, if you know how to set about it. Hold a piece of fresh healthy stomach between finger and thumb, lightly, so that the layers which form it may be distorted as little as possible from their natural position by squeezing. Then with a sharp pair of scissors shear it off, with a flat edge projecting just above your finger ends. Wipe and wet your scissors, and then shave off as fine a paring as possible, and lay it on the glass with a needle. Add a drop of water, and cover the specimen with a thin lamina of glass, remembering which way, upwards or downwards, you have placed the free surface of the membrane. Hold this up to the light, and even with the naked eye you will be able to discern that it is striated with fine lines, more opaque than the interspaces between them. At the base of these striæ lies the transparent corium, and beyond that the still looser areolar sub-mucous tissue with its longitudinal bloodvessels.

Place it under a glass of low power, say of about thirty diameters, and the striated part looks, if perfectly healthy, like palisading, or a collection of short staves, somewhat fatter and shorter than policemen's truncheons, of nearly equal length and packed quite close together, with a very small quantity of more transparent substance between them. These are the "gastric follicles," which in man are simple tubes of the appearance above described, but in animals divide into two, three, or more secondary tubes, and present lateral bulgings, as figured in Professor Kölliker's 'Mikroskopische Anatomie,' figg. 221 and 222, so as to destroy the truth of our rather rough comparisons as applied to the inferior creation.* Under a higher power the general effect is lost, but the contents of the glands are discerned to be a granular substance, which entirely fills up the interior of the glands. Squeeze the cover glass, and the grainy substance oozes out, still slightly cohering in a mass, sufficiently to show that it is the contents of a pipe-shaped follicle.

It is probably the thick secretion oozing from the glands which has led to the notion that the mucous membrane of the stomach has papillæ like the tongue or bowels. Dr. Beaumont speaks of seeing, in the stomach of the well-known Canadian voyageur, "*papillæ* protruding from the mucous coat, from which distils a pure, limpid, colourless, slightly viscid fluid," on the application of food or other stimulus. There is nothing in the organ discoverable by a microscope at all answering this description, except the casts from the glands, which we must suppose Dr. Beaumont saw thrown off on the application of an irritant.

Even in the glands, where it is closely massed together, the secretion gives the idea of containing globules; and if it is separately examined with a high power as it is found on the surface, these are distinct enough. They have been called "stomach-cells," or "rennet-cells," and have a nucleus and nucleolus. Besides this, there is a quantity of grainy matter

* In the human subject also, close to the pylorus, the stomach-glands have an arborescent appearance, like those of dogs or oxen figured in the work quoted. Mr. Ecker has described and figured them as such since the publication of Professor Kölliker's book. Henle's Zeitschrift für rationelle Medicin, N. F. ii. 243.

in the secretion, which is just like the grainy matter which can be seen also in the cells, and therefore is considered to be the *débris* of some of those bodies broken up. If you are examining the stomach of an animal which has died while digestion was going on, you will find these stomach-cells in considerable quantities. I have seen them in the human stomach forming a multitude of little grey flakes, and they are described by Dr. Frerichs as constituting sometimes a continuous white layer to the naked eye.

Now, whether these stomach-cells are the gastric ferment, or rennet, or whether they contain the rennet, or are simply the *concomitants* of the rennet, is truly, at present, a matter of opinion; but they very clearly have a most intimate connexion with it. When they are present, the gastric digestion is going on; when gastric digestion is not going on, they are absent. It appears to be a matter of certainty that from the gastric glands comes the principal portion of the solid animal matter of the gastric juice, the exciting cause of the digestion *in the stomach* of albuminoid substances, the main support of life.

The reason for italicizing "*in the stomach*," will appear afterwards. But it is obvious that these glands are of great importance, and that any observed changes in their appearance must possess much interest.

It is, then, a striking fact, that of the 100 cases examined by Dr. Jones, but 28 exhibited an approach to typical stomachs, the destruction of the tubular glands being in 14 very great—so as probably to render them quite unequal to their duties—and in the remaining 58 being undeniable under the microscope, but not likely to have interfered with the health of the patient.

This condition may very justly be called GLANDULAR DEGENERATION of the stomach, and is by far the most common of the organic lesions which are to be found in that part. To account for its very singular frequency, it must be remembered that nearly all the bodies examined were those of broken-down hospital patients, with all sorts of diseases and degenerations of other parts; and that it would have been strange indeed if the central viscus had not suffered along with the rest of the body. Indeed, it may be doubted whether this amount of microscopic glandular degeneration in the stomach is greater than might be found by a similarly minute examination of the kidneys, or other great manufacturing organs. All that it proves, then, is, that of all parts the stomach has the truest sympathy, in the strict sense of suffering with them, for the other members of the body.*

Glandular degeneration in the stomach, as elsewhere, involves two morbid actions—to wit, *imperfect growth of tissue*, and the *growth of imperfect tissue*. There is an absence, more or less complete, of the natural substance in its proper form, and the presence, more or less overpowering, of a substance possessed of lower organization, less life. The greater or less comparative predominance of one of these parts of the idea of Degeneration, gives birth to the infinite variety of forms under which it is pictured. The complete type of one extremity of the scale is the utter

* There is a volume, published by a Dr. Rigā, in 1621, with the title 'De sympathiā, sive consensu membrorum omnium corporis, et præcipuè stomachi.' The doctrines propounded are very like those of Broussais.

destruction of the tubular glands, and the substitution of nothing in their place; and at the other extremity would lie the deposition of granular substance, and no apparent wasting of the forms of the tubes. The exact medium is an equal proportion of wasting and granular matter, so that the weight and measure of the organ is not altered.

The longer and more attentively that degeneration is studied, the less real differences there appear to be between the three forms of fibroid, albuminoid, and fatty matters, which are substituted for the properly-formed tissue of glands or other parts, when the above-mentioned change takes place in the nutrition of the part. They are all evidences of a lower degree of interstitial life than ought to exist. Though different for the morbid anatomist, they offer to the physician completely identical suggestions, both for prognosis and treatment.

Besides the degeneration of the glandular and interstitial substance, Dr. Jones also notices the wasting of the epithelium alone, in some cases, which may be considered a minor stage of the more complete lesions above noticed.

It will easily be judged, therefore, that any classification of the appearances assumed by degenerated organs must be a purely arbitrary one, dependent entirely on the greater or less tendency of the observer, artist, or writer, to minute division or broad generalization. Bright's disease of the kidneys affords a striking instance of this, each author dividing its pathology differently; and acting so far wisely; but often, in the sequel, provoking a smile by an unphilosophical attack on the divisions of his collaborators. It is gratifying to see that Dr. Jones, though he has not in words set forth the doctrine of Degeneration above stated, yet in fact shows that it is influencing his thoughts by making no attempt at classification of the forms produced by its doings in the stomach.

Like all degenerations, that of the gastric glands is more prevalent as age advances—indeed, it would appear that only before full growth is attained can we safely calculate on finding these glands quite perfect in the stomach of an invalid. Yet the degeneration is not merely a physiological development; that which increases with age is not, as might be suspected, age itself; for in some few of the oldest cases examined by Dr. Jones—in one particularly, who had attained to seventy-four—the tubes were quite healthy. The probable explanation is, that the longer a man lives the more likely he is, on the mere doctrine of chances, to have some disease which leaves its traces in a microscopic injury to the tissues. The longer a man lives the more likely he is to feel half a dozen earthquakes, but his age is not the cause of the earthquakes.

As in degeneration indefinitely-shaped fibrine is formed in the tissues, and as in inflammation indefinitely-shaped fibrine is thrown out and fibrine also exists in excess in the blood, the two processes have been likened to each other, and the one at present under discussion has been shortly disposed of as "inflammation." But how different the vital phenomena in those almost contrasted processes! Watch degeneration—where you have an opportunity of watching, so well all physiological acts—in the transparent eye. If arcus senilis were accompanied or preceded by congestion, heat, swelling, and pain, such facts could not escape notice; but there is notoriously nothing of the sort. If Bright's disease of the

kidneys began with inflammatory symptoms, we should not be left long doubting about the fact. True there is sometimes hæmaturia, but certainly not as a rule; and that hæmaturia is not of an active sort. There is none of the pain, fever, and vomiting, well known to accompany true renal inflammation.* Then, again, degeneration attacks principally those parts which are least liable to inflammation, such as the kidneys; and avoids those which are most liable, such as lungs, areolar tissue, and skin. It is surely a pity to use any common term for such very different things.

On the causes of degeneration of the gastric tubes, Dr. Jones's cases do not enlighten us much. The lesion appears associated with all sorts of diseases, as well as with the diseases of all parts. The universal "sympathy" of the stomach seems most impartial. Some influences that moral grounds might induce us to hope would be shown to act in the production of the lesion, do not seem to have much to do with it. The 11 cases of hard drinkers had, as a rule, less the matter with these glands than the temperate. In only 3 was the amount of destruction very extensive, and in one man, whose addiction to the vice was so great as to sink him materially in the social scale, and cause his death in an hospital at forty-nine of diseased heart and liver, there was unexpectedly found a very tolerable state of stomach—not more disease, in fact, than was observed in numerous patients whose lives had certainly been very unlike his.

We come now to the main point—What effect upon the bodily economy has the loss of the services of this portion of membrane? Do we depend upon it entirely for the digestion of our meat, and are we therefore to give up the chance of supplying necessary albumen to our tissues in proportion as these glands are wasted? It would at first sight appear as if no gastric juice could be secreted, and therefore that no flesh meat could be made of use to the patient.

Now, in the first place, attention must be called to the fact that the semi-solid matter formed in these glands is by no means the most prominent ingredient in the gastric juice. Water constitutes its chief bulk, and, there is every reason to believe, is a most important ingredient therein. It is being continuously poured forth in vast quantities from the surface of the membrane, and neither the microscope nor the analogy of other glands, such as the kidneys, would lead us to think that the secretion of water was at all arrested in glands by the process of degeneration. Indeed, in many cases of advanced renal disease it is augmented.

To see the importance of this aqueous secretion, which we suppose to be undiminished, we must reflect on its very great quantity in the healthy, and, as above suggested, probably also in the unhealthy state. Drs. Bidder and Schmidt, from experiments on dogs with gastric fistulæ, inferred that in carnivorous mammalia the gastric juice secreted in twenty-

* The writer has had lately one of those opportunities which so rarely occurs, of keeping under his eye the whole course of a case of degeneration of the kidneys. He was obliged to have tracheotomy performed on a sailor with ulceration, probably syphilitic, of the larynx. The urine and everything else continued quite healthy during the four months it was necessary to keep the trachea open for the cure of the larynx. But unaccustomed confinement to the house, and perhaps the remedies used, broke down his constitution: he got dyspepsia, albuminuria, dropsy, and fatal diarrhœa. After death, the kidneys were found to be double their natural size, and quite yellow with morbid deposit. At no time was there pain in the loins, nor did the urine ever contain blood or fibrinous casts.

four hours equals at least one-tenth of the animal's weight, and that of this, ninety-seven per cent. is water. Assuming the same to hold good of our own species, a man of ordinary size would daily secrete from fifteen to sixteen pints of this very aqueous fluid from the limited area of his stomach. It is curious to see how loath philosophers seem to receive this undeniable inference from Dr. Bidder's facts; even Dr. Jones, in the work under review, quotes the opinion with evident caution (p. 30), and seems to think that dogs are no rule for men. People view themselves as almost insulted by the accusation of having all their life been doing such an extensive business without knowing anything about it; and Dr. Lehmann, by arguments derived from chemistry, would prove that four pounds *per diem* is quite enough for a man to make.

Great interest, therefore attaches to an opportunity lately afforded to Dr. Grünewaldt of measuring by actual experiment the gastric juice of our race, in an Esthonian peasant with a stomach fistula, which had existed from childhood, and did not in the least degree interfere with the general health.* If astonishment was excited by the large figures of Drs. Bidder and Schmidt, how must it be increased by this confirmation of them in our own species, who appear, from Dr. Grünewaldt's experiments, to secrete not one-tenth, but between a fifth and a quarter of their weight in gastric juice daily!

What is the office of all this water? It forms part of that great circulation through the alimentary mucous membrane, which might truly be called the fountain of animal life. It is constantly going its rounds like an endless chain, going out poor, and returning in a continuous stream laden with wealth; and is thus carrying on business with the outer world quite as important to existence as the more generally known and more anatomically distinct circulation of blood.†

It is difficult to over-estimate the importance of the watery part of the secretions of the alimentary canal, and we must not think that when the power of forming the peculiar solids of the gastric glands is taken away, that all the office of the stomach is annulled. The water still exudes, and not improbably carries with it some imperfect fluid-representative of the "rennet cells."

Besides this, were digestion in the stomach to be completely stopped, there appears to reside in the intestines a power of taking the duties refused by their leader. Drs. Bidder and Schmidt, by experiments on living animals, have shown that albuminous matters inserted into the ileum, with all access of gastric juice cut off, were dissolved in the same way as in the stomach;‡ and the repetition of these experiments by Dr. Jones have fully confirmed their correctness.

Further experiments, too, by Dr. Ernest Schröder, render it most

* *Succi gastrici humani indoles physica et chemica ope fistulæ stomachalis indagata.* Auctore Otto a Grünewaldt. Dorpat, 1853.

† The germ of the idea of a circulation through membranes, may be found in that strangely-worded collection of suggestive thoughts, Emmanuel Swedenborg's 'Animal Kingdom' (chap. xvi. § 325). He applies it to serous sacs, for the state of physiology in his time did not enable him to see its applicability to the mucous membranes. The unintelligible language in which they are couched has concealed this, and perhaps still conceals many another brave idea.

‡ *Die Verdauungssäfte.* Fünfter Abschnitt. Vom Darmsaft.

probable that in the human species this digestion of albuminoid matters by the intestines is not merely a power occasionally exerted in case of accidental arrest of the stomach's functions, but the normal action of the alimentary canal. He finds, by inspection of the Esthonian peasant, also observed by Dr. Grunewaldt, that albuminoid matters are never entirely digested in the first receptacle. In dogs, indeed, as he found by a repetition of Drs. Bidder and Schmidt's experiments, complete solution and conversion into peptone was always to be attained; but in the human patient, though in better health than the dogs, unaltered muscular fibre and the like always passed through the pylorus, and was never entirely absent from the contents of the stomach.*

It is clear, therefore, that any diseased state of stomach coming on slowly may be compensated for by increased vigour of intestinal digestion, and that in the human species this compensation is the easier attained because it is, to a certain extent, the normal condition.†

The loss of the services of the gastric glands is, then, less immediately influential on the vital acts than would have been deemed at first sight; and we shall not be so much surprised, after this introduction, at the evidence which runs through the whole table of cases given by Dr. Jones in the 'Medico-Chirurgical Transactions,' of the vague character of the symptoms referrible to the lesion. The effect on the bodily economy is so slight, and so easily masked by the least accompanying ailment, that it is rare in the history of the cases to find any evidence, of its existence, even when the degeneration is very extensive. Neither does it shorten life to any appreciable extent; the average age of the patients with healthy stomachs was fifty-two, and of those with degenerated stomachs fifty-one. In only one case, a man of sixty-two (*No. 2 in the Table*), was the lesion the sole cause of death; and it did not prevent a workhouse nurse (*No. 69 in the Table*) from attaining the age of ninety.

Sometimes, indeed, there are notices in the history of the patients of weight at the pit of the stomach, a stoppage of food at the bottom of the gullet, occasional vomiting, unaccountable anæmia or debility, and a difficulty of rallying during acute disease; but in the great majority it is satisfactorily shown that there were no gastric symptoms at all to evidence any but very decided cases. And in one non-tabulated observation,‡ where a woman who died of burn exhibited after death very great destruction of the tubes, there was no derangement of digestion, and the patient could eat any kind of meat.

Is it not probable that the differences between the two classes of cases, where symptoms were exhibited and where they were not, lies in the condition of the rest of the alimentary canal? Where the stomach is injured, even to a great extent, and the succeeding parts remain healthy, does not physiology teach us to expect compensation and little derange-

* *Succi gastrici humani vis digestiva.* Auctore Ernesto de Schröder. Dorpati, 1853.

† The writer of the present article has been in the habit of using, with great success, as a main article of diet, in cases of gastric derangement, of low fever, &c., milk, prevented from coagulating by the addition of one-third of its bulk of lime-water. (An instance of the practice is quoted by Dr. Jones from the writer's clinique, in p. 61 of the book 'On the Stomach.') This has been objected to as impeding the natural digestion by the gastric juice. The answer is, That is the very thing required—the stomach is weak, so we wish to spare it. The stomach is short, and in sickness digestion is long; so we pass it on to the long bowels.

‡ *Medico-Chirurgical Transactions*, p. 106.

ment? But where the whole of the digestive mucous membrane is even slightly affected, are not considerable disturbances of function to be looked for?

• We trust, then, that Dr. Jones's most valuable series of microscopic observations on the degeneration of the stomach will be followed up by a corresponding examination of the small intestines. •

The next most frequent morbid condition of the mucous membrane of the stomach is that which is called in the 'Cyclopædia of Practical Medicine,' "Follicular Gastric Dyspepsia;" by Cullen, "Anorexia Humoralis;" by Dr. Jones (after the Germans), "GASTRIC CATARRH." The first name is anatomically wrong, for it assumes that the mucus is formed from the gastric glands or follicles—which is not the fact; Cullen's nomenclature is too narrow, for the appetite is not always deficient; and Dr. Jones's is too broad, for it includes the acute condition (the *embarras gastrique* of the French, our *bilious attack*) along with the chronic, as if one was a sequel or continuation of the other. Now this is a thing very much to be avoided, as it will introduce into the pathology of digestion all those false notions and false practices which have been occasioned by the unfortunate common name of "bronchitis" for an acute cold on the chest, and for a chronic secretion. We must confess to a prejudice against *omne quod exit in "itis,"* and would propose to use the word "catarrh" for the conditions, gastric and pulmonary, which run an acute course, *tending* to get well of their own accord; and the word "flux"—gastric flux or bronchial flux—for those whose course is chronic, i. e., which tend to confirm and aggravate themselves, to get worse unless counteracted.

In twenty-three of the hundred cases examined there was the above-named excessive secretion of mucus in the stomach; and from these twenty-three cases Dr. Jones makes the following deductions:

That age disposes to it;

That the two sexes are about equally liable;

That it is not dependent or in any way connected with degeneration of the tubes;

That drinking does not dispose to it;

That it is in the majority of cases associated with augmented secretion from other mucous systems.

In discussing the symptoms arising from this state of stomach, Dr. Jones has himself given an example of the confusion likely to arise from his name of "catarrh;" for he speaks of the chronic state, as the "later period of catarrh, when the hyperæmia has subsided."* But no proof exists that chronic mucous flux begins with hyperæmia; and it cannot but lead to bad pathological inferences and bad practice to assume such a doubtful point.

The description of the symptoms is evidently drawn from nature, without any of the filling in of the rough outlines which renders our systematic works so smooth and flowing; and it is, therefore, doubly valuable to those who appreciate truth. Among them the author has of course not failed to enumerate pyrosis, and so far we cordially agree with him. But we cannot avoid dissenting from the etiology which he

* On the Stomach, p. 173.

assigns to this latter complaint. He views the fluid thrown up as a mere variety of gastric flux, as mucus with its viscid nature diminished and its aqueous portion very much increased, thrown up from the stomach by reason of its superabundance. Now there is no evidence that "water-brash" has ever been in the stomach at all. The effort by which it is ejected is quite different from vomiting, and much nearer resembles that which takes place in obstruction of the gullet by cancer, ulcer, or contraction; there is no heaving of the diaphragm or spasm of the abdominal muscles, such as is necessary to empty the stomach, but an easy "rising" of the fluid into the mouth at a time remote from meals. Then, again, the matter thrown up contains generally none of the mor- phic or chemical elements peculiar to the stomach, no stomach-cells, or gastric ferment, or muriatic acid; but, on the other hand, exhibits the characteristics of saliva. It is alkaline, opalescent from the presence of the pavement epithelium of the mouth and throat; and, according to Dr. Frerichs,* converts starch into sugar, and contains cyanide of potassium. Is it not likely to be the secretion of the salivary glands, mouth, and gullet, detained in the latter by spasm, or, at all events, the saliva swallowed and detained in the stomach, rather than a watery flux of the gastric mucous membrane?

It is true that sometimes water-brash occurring at a period soon after meals, is of a different character from that above described, being intensely acid, and exhibiting some of the usual contents of the stomach. But this seems to be rather an accident than a rule, and should probably be distinguished from the pure ailment, and be designated as water-brash complicated with vomiting—the said vomiting being of a slight character, and excited by the proceedings of the œsophagus.

Pyrosis is indeed a symptom of gastric mucous flux; but it is by no means peculiar to that derangement of the stomach occurring in cancer of the cardia or pylorus, in ulcer, and sometimes in hysteria—being, if the explanation above given be correct, a secondary disorder exhibited rather in the healthy than the ailing part.

SARCINÆ; though absent from the vomit during life, were found in two bodies (Nos. 33 and 67 of the Table). A remark, omitted by Dr. Jones, probably for the sake of condensation, may here be inserted—viz., that the first case, a patient of the present writer's, exhibited a proof of the true habitat of this parasitic mould; the sarcinous cells lay in considerable abundance, strongly adherent to the external layer of a gluey mucus, but none stuck to, or even touched, the membrane; thus showing that it is in some cases, and probably in all, a parasite accidental to an excess of tough secretion, and not connected with any deep-seated change. The gastric glands in this instance were normal.

ULCERATION is another lesion of the stomach to which Dr. Jones assigns greater frequency than is usually known. In the 100 consecutive cases tabulated, there are six of ulceration. The reason of its not being known to be so often present, probably is the rarity with which the stomach is opened, unless there are ~~any~~ special symptoms directing attention to it, and the great chance there is of the latency of these special symptoms in cases fatal from other causes.

Dr. Jones differs from Professor Rokitsansky in not viewing "perforat-

* Frerichs, in Hüser's Archiv, x. 175—208; Lehmann's Phys. Chemie, ii. 130.

ing ulcer" as a distinct class. He says he has not "been able to observe anything to distinguish them from other ulcers"—i.e., from such as do not perforate. In this he is fully concurred with by the writer of the present article, who published in the 'London Journal of Medicine,' three years ago, a statistical collection of 22 cases of gastric ulceration, from autopsies made at St. George's and St. Mary's Hospitals, in which the same doubt is thrown upon Professor Rokitansky's classification, and the perforating character attributed, not to anything in the nature of the ulcer, but to all ulcers which occur in youth.

"The same tendency to strike deep and perforate, exhibited by ulcers in youth, is equally seen in ulcerations of other parts of the intestinal canal besides the stomach, and moreover appears quite independent of the disease which has given rise to the ulceration; it is the young and vigorous whose intestines become perforated, whether the first origin of the evil was pulmonary consumption, dysentery, fever, or chronic inflammation. This fact is proved by the same decennial collection of 2161 post-mortem examinations at St. George's Hospital, which I before quoted. In 128 of these, various parts of the intestines, not including the rectum, were ulcerated, in connexion with tubercular disease in the lungs and elsewhere; and in 9 of the 128, perforation had taken place. The ages of the 9 were as follows, placed in order of their youth—namely, 15, 17, 22, 22, 25, 29, 33, 34, and one young man of age unknown. In 80 other cases, non-malignant ulceration had occurred from various causes, chronic and acute, independent of tuberculosis; and perforation had occurred in several parts (exclusive of the rectum) in 16 instances, as a consequence of the extension of the ulcer outwards. The ages of these 16 patients were, 7, 16, 16, 16, 20, 21, 22, 24, 24, 28, 30, 30, 35, 37, 56. These examples suffice to show exactly the same fact as our table of ulcerations of the stomach—namely, that *all* ulcers tend to penetrate the peritoneum more in youth than in declining years, but that such a tendency is not by any means confined to one period of life, nor to one peculiar species of ulcer."*

Ulceration of the stomach seems to be a state entirely independent of the degeneration before noticed:

"The tissues bordering the ulcer have not presented anything constant or to be specially noticed; sometimes they appear tolerably healthy, sometimes they are diseased in the same way as other distant parts, sometimes they are the seat of blood congestion, but this is not often the case."†

Neither does it appear connected with increased mucous secretion; for though that condition is occasionally present in ulcerated stomachs, it is by no means constant. Indeed, since ulceration is rather a disease of youth, and degeneration and mucous flux belong to old age, there is a sort of antithesis in their pathological history.

Yet both ulceration and degeneration are connected with depressed vital powers, but in the former the cause is acting acutely and destructively, in the latter slowly and with the deposit of compensating tissue.

BLACK MATTER‡ is so frequently found among and in the gastric glands, and, indeed, throughout the whole intestinal mucous membrane, that it becomes a matter of considerable interest to know what it means. Some boldly and vaguely assert that it is an evidence of former "inflammation,"

* On Ulceration of the Stomach, and Ulceration of the Oesophagus, by Thos. K. Chambers, M.D.: London Journal of Medicine, July, 1852.

† Medico-Chirurgical Transactions, p. 97.

‡ Sometimes unphilosophically called "pigment," as if its whole business was to daub the tissues for the amusement of anatomists. One might as well speak of the lily being painted white and the trees green by Nature's colour-box.

and if they agree to use inflammation in the very broad signification given to it by Professor Virchow, viz., any structural change with or without exudation, they are probably right. But if increased afflux of blood, tumefaction and exudation are to be viewed as necessary parts of the process, they have no evidence in the case before us that this necessarily occurs when black matter is seen. Dr. Jones well remarks:

“The very frequent formation of black pigment in the interstices of the air-cells of the lungs, in the substance of various tumours, and the frequent occurrence of chloasmata in the skin, are sufficient to show that abnormal pigmentary deposits are no very wide or extraordinary deviations from the type of healthy nutrition, and that they are by no means necessarily dependent on hyperæmia.” (p. 116.)

Dr. Jones is inclined to view black matter as an evidence only of the exudation of fluid containing hæmatine.

About SOFTENING of the stomach our author has no new observation to make, except that the parts of the membrane undissolved show no deviation from the healthy structure, even under the microscope.

THICKENING OF THE SUB-MUCOUS COAT, he follows Andral and others in thinking, has often been mistaken for cancer, and so the number of malignant diseases unfairly swollen. Such may have been the case; for long-continued ulceration, especially if complicated with local inflammation and adhesion of the peritoneum, will sometimes produce very great increase of bulk in the subjacent and neighbouring fibrous tissue. But on the other hand, have not some cases of a real cancerous nature been set down as mere thickening since the observations of Andral? It is by no means uncommon to find in corpses, dead of indubitable cancer, a thickening of the pylorus and neighbouring parts, which, without the distant collateral evidence, no microscopist or pathologist could possibly pronounce to be malignant. If the stomach chanced to be the only part affected, would not this be accounted non-cancerous hypertrophy?

Where are the limits of cancer? What is its essential difference from other abnormal tissues? When Dr. Jones describes the new-formed substance in thickened stomach as “manifestly a growth, augmenting by assimilative power its own tissue, just as a normal tissue does,” is he not encroaching on the definition of cancer? Is there any real line of demarcation between it and fibrous tumour? and is not malignancy a question of degree, of more or less cancerous nature, and not of absolute difference? Dr. Jones seems by no means disposed to follow the bare path of imitation in his pathology, and we should have been glad to have seen him take the opportunity presented by the juxtaposition of “thickening” and “cancer,” to have placed some notions at the public service. That the so-called “cancer-cell,” as a distinctive mark of malignancy without regard to quantity, must be given up, is becoming evident, and suggestions on the subject are much required.

The ground hitherto travelled over has been the same in the two works quoted at the head of this article—viz., the paper in the ‘*Medico-Chirurgical Transactions*,’ and the volume which followed it; consisting of deductions made almost entirely from the 100 consecutive post-mortem examinations. In the separate volume there is added a chapter of “*Clinical Observations*,” and some more detailed histories in the previous

pages of the work. These are clearly and graphically written, and exhibit well the difficulties which a practitioner has to contend with in the diagnosis of gastric symptoms. Perhaps it would have been better if the name of the physician who attended the patient had been appended in each instance; but in spite of the want of such aid to identification, the writer of the present article has been able to recognise some of his own patients by the history alone—no slight proof of its general correctness.

In the treatment detailed, the most important point seems to be a more general use of astringents than is common in gastric cases. Tannic acid, nitric acid, nitrate of silver and alum,* are freely employed, and with obvious benefit. We should have learnt more of the efficacy of the various drugs had they been given separate, instead of in compound prescriptions. Nitrate of silver, for instance, lies under the serious imputation of being rendered inert by saliva and mucus long before it gets to the stomach, and certainly clinical experience goes far to confirm the accusation. Dr. Jones often gives it, but never *alone*, so that one cannot say whether the benefit which follows is due to the drug or not.

Another important point, to which attention is properly called, is the use of mercury. It may be doubted whether the author does not overdo this, in actually producing salivation, but no one who has tried it can doubt of the efficacy of alterative doses of the drug, such as a few grains of grey powder every night. It probably acts by augmenting the destruction of effete tissues, especially of the blood, and so promoting healthy secretion and absorption. And of course the beneficial alteration is first seen in the most failing part, in this case in the stomach.

Leeches to the epigastrium are also very efficacious in many gastric complaints, and may without inconsistency be combined with astringents. They act probably partly as alteratives, improving the blood by removing effete matters, and promoting absorption of new; but at the same time they relieve the morbid congestion of the viscera beneath the point of application. Applied in moderation, they do not seem to reduce the vital powers. The reviewer had lately under his charge a young woman to whose epigastrium he thought it desirable to apply four leeches every night, yet she continued all the time to increase in flesh, and during one week gained four pounds in weight.

The extracts made from Dr. Jones's work are sufficient, probably, to justify our readers' hope to see him again in this department of science, employed, as now, in illumining the darker regions of pathology with the modest light of truth, instead of the *ignis fatuus* of speculation.

Thomas K. Chambers.

* The present writer has used with great success in a few stomach cases lately, "iron alum," in doses of from three to six grains. It fulfils the double indication of a powerful astringent and of a martial tonic, peculiarly adapted to the anæmia usual in chronic instances of these lesions.

REVIEW III.

On the Nature, Signs, and Treatment of Childbed Fevers, in a Series of Letters addressed to the Students of his Class. By CHARLES D. MEIGS, M.D., Professor of Midwifery, &c., in the Jeafferson College, Philadelphia, &c., &c.—*Philadelphia, 1855.* pp. 362.

No one who has suffered in his own practice from an incursion of puerperal fever is ever likely to forget the distress it occasioned. The contrast between the pleasure of a safe delivery, followed by a few days of peaceful convalescence, and the fearful doubt and misgiving excited by a rigor, confirmed by succeeding fever, pain, and prostration, and terminating but too frequently in hopeless, helpless distress, is too striking easily to be obliterated. The misery of finding that every remedy fails in arresting the progress of the disease, the sad contemplation of the future, the present sorrow of the family, the sense of desolation and impending calamity which pervades the house, the hushed voices, the quiet movements, the daily and hourly appeal to the doctor for some more favourable news, and the sad reply, increasing in anxiety as hope diminishes, until question and answer wring the hearts of both parties with an anguish, as impotent as hopeless.⁶

All this many of us have felt in our inmost hearts, and thus suffering, have been ready to wish that we were "hewers of wood and drawers of water," rather than members of a profession at once so responsible and so powerless. If, in addition, it be at all possible that the medical attendant may have himself conveyed the infection to his patient, we need say nothing in explanation of the deep and enduring distress he must feel.

It is not wonderful, therefore, that the subject of puerperal fever should possess an interest amounting to fascination, for the practical obstetrician, nor that he should eagerly peruse any new work upon the subject. And yet, upon the whole, the result of very extensive reading is far from satisfactory; authors of equally high reputation, and equally extensive experience, are found to differ as widely as possible upon the nature, causes, and treatment of the disease; nor, we must confess, are the views of most of them either so logical, or of such breadth and soundness, as to claim our ready assent; so that in the end we are left either to reconcile their differences in the best way we can, or to make a selection of one side or the other. In truth, it cannot be denied that the thorough understanding of the subject is far from easy, even after much reading and consideration, and some experience; there are facts which cannot be explained by any theory; others, apparently directly opposed to each other, are related on equally reliable authority. It is possible that the inductions to be drawn from the facts recorded require more caution and thought than has hitherto been exercised upon them, and perhaps a wider acquaintance with pathology in general. Lastly, the reader may reject the hostile criticism of opposing authors as being, in most cases, worthless, inasmuch as different epidemics assume different or even opposite pathological characters; an epidemic of puerperal fever will always partake more or less of the prevailing character of disease at the time, so that

two writers may be describing the same *nosological* disorder, though practically and pathologically a different disease; the opinions and practice of the one will be at variance with the experience of the other, although strictly correct and sound as regards his own.

We would wish to impress upon our readers this important fact, as a check upon a shallow and exclusive system of reasoning, and as the only possible way of reconciling the contradictory descriptions of puerperal fever—viz., that *the prevailing constitution of disease influences an epidemic of puerperal fever, so that different epidemics may exhibit different or even opposite characteristics.*

This axiom is now so incontestably established, that we entered upon the perusal of the work at the head of this article fully expecting its recognition by the author, and anxious to examine, from his point of view, the epidemics he has witnessed in America. In this expectation, however, we have been sadly disappointed, as all his reasoning is based upon the pathological identity of the disease under all circumstances.

No one can deny the eminence of Dr. Meigs' position, nor the influence he wields on the other side of the Atlantic; as little can any one doubt his great experience, shrewd talent, or facility of writing, although his style is spoiled and made quite barbarous by the use of new, foreign, and bombastic phraseology. We concede to him at once, and willingly, that which he claims—the right to form and to maintain his opinions, to see things with his own eyes according to the light which is in him, and to state decidedly his own views, no matter from whom he may differ. Nay more, we accept his facts—i. e., his descriptions of disease as he saw it, although we may neither agree with his reasoning nor with his generalizations. We do not, however, yield to him the right to speak disparagingly or disbelievingly of the opinions of others, because they differ from his own, nor to dispose of their evidence *ex cathedra*.

Claiming the "right of private judgment" in its fullest extent, Dr. Meigs sometimes forgets that an equal right exists for his opponents. Frankly and fully admitting and claiming this right, we shall without scruple examine his opinions with freedom and courtesy, but without hesitating to express our dissent from many of them.

The volume is written in the form of letters to his class, as affording greater freedom of expression; but essentially, it is divided into the consideration of "milk metastasis," the state of the blood in childbed fevers, contagion, description of the disease, etiology, diagnosis, and treatment. The chapter on milk metastasis is of interest historically, as showing how easily an assumption may be mistaken for a fact, and how the error is perpetuated by successive writers. It is executed very ably, but we cannot agree with what seems to be the conclusion of the author—viz., that because the opinion that the blood in certain puerperal attacks was rendered impure by the transference of the milk, is unfounded, therefore that all supposition of blood deterioration is equally baseless. Such a notion is rejected and ridiculed in many parts of the book, and is treated as quite inconsistent with the author's pathological views.

"What difference does it make to you," he observes, at page 79, "whether our class shall receive and adopt the milk dogma of Thomas Willis and the Frenchmen" (the "milkmen," as he calls them in another place), "from Goubelly to

Puzos, and from Doublet to Vigarous: or whether they shall prefer to fill the blood with various products of absorptions and resorptions, and then attribute to the noxious presence of those putative foreign matters a host of diseases, whose evolution is made by them to depend upon these alone or in chief?"

But Dr. Meigs' opinion on this subject does not seem so much the result of a careful examination of facts, as the consequence of a preconceived theory of disease, which we are told has prevailed for some time in Philadelphia, and one of whose ablest champions was the late Professor Chapman. Thus, when considering the state of the blood in childbed fevers, he commences by explaining his views of the mode of blood-formation in the fœtus in utero, which he attributes to the endangium, or blood membrane, and whose inflammatory condition (endangitis) is the only possible way by which the blood may be deteriorated. Then passing on to the state of the blood during gestation, he observes:

"A woman pregnant is often observed to labour as to her circulation; she becomes sometimes plethoric and hyperinotic, at other times she is to the last degree hydræmic. The force of her hæmatisis is exaggerated or exhausted, as the case may be, and the direct fault, the pathological fault, is to be traced to a state of the endangium, which is her blood-making tissue, her blood membrane, and which has the same relation to the function of hæmatisis as the gastro-enteric mucous membrane has to the process of her digestions. If the digestive organs become diseased, the power of digestion fails proportionally; if the hæmatisic tissue becomes diseased, the hæmatisis fails in like manner. We habitually speak of the digestive mucous membrane and of the respiratory mucous membrane,—blood membrane is equally a true and honest word, one that conveys an accurate and concise meaning or idea. Well, then, when I speak to you of blood in diseases as depraved, as vitiated or dissolved, as ruined, and incompetent to carry on the functions appurtenant to it—as the antagonism of the solids—I desire you to understand me as speaking of diseases of the membrana communis (endangium). I do not more conclusively refer the dysenteric diarrhœas, croups, &c., to a state of the gastro-intestinal, pulmonary, or laryngeal mucous membrane, than affections of the blood to affections of the blood membrane—endangium.

"All scarlatinas, measles, variolas, varicellas, crÿsipelas, gout, rheumatism, and many forms of childbed and other fevers, have their prime seat in the blood membrane (endangium), and are but so many varied expressions of its diseased conditions." (p. 77.)

We have introduced this quotation, that our readers may learn from himself the basis of all Dr. Meigs' pathological views, and not in order to discuss their merits, because it appears to us that whether his opinion of the first step of the morbid process be true or not—and we confess that we cannot regard it as the truth—does not materially affect the most important question at issue, which is rather the constitutional and local effects of depraved blood, no matter by what means it has become impure.

Our limits necessarily prevent our going through the volume *seriatim*; we must therefore select a few of the more salient and important of the questions discussed, and confine our observations to them. Such we may well consider the nature of puerperal fever, its communicability or contagion, and its treatment. By way of giving our readers a *carte du pays*, we may state shortly, that Dr. Meigs denies totally the existence of a primary puerperal fever; he regards the disease, so called, as a local disease with constitutional symptoms, in all cases; he laughs to scoru

all notion of contagion; and deems large bleeding the only effectual remedy.

I. Let us now inquire what is the nature of puerperal fever, and whether, under all circumstances and in different epidemics, it is always pathologically the same? Is it always a primary local affection with consecutive fever, or may the fever in some cases or epidemics be primary and the local affection secondary? Is it always of a sthenic-inflammatory character, or occasionally adynamic and typhoid; and if so, are we to assume an identity of morbid process in both instances? Dr. Meigs announces his own opinions clearly enough. For example:

“At present the question is, is there such a thing as a childbed fever? To this question I am compelled to answer in the negative; wherefore I must consider the word a false and misleading one, since it implies that the disorder is a fever, when, in fact, it is not a fever, but a phlegmasia, or pure inflammation.” (p. 121.)

Again:

“Why, this is truth, that childbed fever is a phlegmasia and not a fever. If you can cure the inflammation, all the rest will be cured *ipso facto*.” (p. 139.)

And again:

“Whenever these inflammations occur in pregnant or lying-in women, then the disease is childbed fever, and it is nothing else; nor are childbed fevers ever anything else, except when they occur as accidents in typhus, small-pox, plague, and other typhous diseases, of which they are in some instances mere complications.” (p. 129.)

The latter clause seems to have been the result of a misgiving that his theory would hardly cover all the cases met with in practice, and it is the only approach in the book to an admission of varieties of disease in different epidemics. The arguments or evidence in support of the author's views consist of quotations from his own experience or that of others, showing that local diseases of various kinds were found, and that they were inflammatory. Now, no one doubts that there are almost always local diseases, and that in many cases they are the result of inflammation. In Dr. Meigs' cases we are quite prepared, on his own showing, to believe that they were so; but this does not prove that it is always the case, nor does Dr. Meigs adduce any adequate evidence to this effect; but he answers evidence and conclusions opposed to his own views with a sneer, speaking of “Doublet, Puzos, Vigarous, Sellé, and the whole host of the milkmen!” and of Dr. Ferguson's valuable essay as “the most misleading and weakest book on childbed fever that has appeared since 1795,” which is neither graceful nor conclusive. Throughout the entire examination of the subject, Dr. Meigs assumes that the epidemics he has witnessed were identical in character with all other epidemics, and consequently that his interpretation is equally applicable to all. Now, this is exactly the point on which we must beg leave to differ with the Professor. As we have already said, the evidence afforded by the carefully-recorded experience of others, and some experience we have had ourselves, has satisfied us that epidemics of puerperal fever share in the changes of constitution noticed in other epidemic diseases, some being characterized by sthenic-inflammatory action in the fever, and local affections, while others exhibit an opposite or asthenic type, and the local affections show very little trace of inflammation; and we give Dr. Meigs the benefit of this wide compre-

hension. We do not doubt his accuracy as regards the cases *he* has seen ; we take his statement without question, that they were acute sthenic local inflammations with consecutive fever, and were cured by bleeding, when early employed. But we can as little deny, if we are to judge according to the evidence, that epidemics of an opposite character have occurred, and that of late years this type has prevailed, in this country at least. In coming to this conclusion, we are obliged to do that which is not particularly agreeable to human nature—that is, avow a change of opinion. Formerly we adopted the views so clearly set forth by Dr. Robert Lee in his valuable papers, nor was it until personal observation had convinced us of their inadequacy, that we admitted the modification we have just stated. It would be easy to quote a host of witnesses to the existence of epidemics presenting different characters to those described by Dr. Meigs, but we must content ourselves with two or three. For example, Mr. C. White, of Manchester, describes the cases he witnessed as being “a malignant fever of the same genus as the gaol or hospital fever.”

Dr. John Clarke, of London, who was among the first to recognise the prominence and distinction of the local diseases of childbed fever, and whose essay is still one of the most valuable we possess, has also a separate chapter devoted to the consideration of the “low fever of childbed,” an affection very different from the acute inflammations which occur at the same period, and which he considers as exhibiting many analogies with typhoid diseases. The local affections he regards as secondary, and the fever as primary or essential.

Dr. Gooch observes, that—

“The most remarkable circumstance that the experience of the last few years has taught us about peritoneal fevers is, that they may occur in the most malignant and fatal form, and yet leave few or no vestiges in the peritoneum after death. The state of this membrane, indicated by pain and tenderness of the abdomen, with rapid pulse, appears to be not one uniform state, but one which varies so much in different cases that a scale might be formed of its different varieties: this scale would begin with little more than a nervous affection, often removable by soothing remedies, and when terminating fatally, leaving no morbid appearances discoverable after death.”

Dr. Meigs' comment upon this statement of so eminent a physician is very characteristic of his mode of disposing of the testimony of an opponent:

“I wholly deny that a woman can have a childbed fever, according to Dr. Gooch's first scale, and should deeply lament to find a student of mine taking up such an idea; since I cannot conceive of his entertaining it, without at the same time admitting a childbed fever to be a *fever*, which is a doctrine both false and dangerous; false as to the pathology, and dangerous as to the practice—that is, false both exegetically and practically.” (p. 143.)

We are satisfied that our American friends will require something more profound and logical than this before rejecting Dr. Gooch's experience. Dr. Douglas states, that judging from what he has himself observed—

“The contagious puerperal fever of Dublin is, I venture to pronounce, neither more nor less than a malignant fever of a typhoid type, accompanied with an erysipelatous inflammation of the peritoneal covering of the stomach, intestines, and other abdominal viscera.”

Dr. Collins has exactly expressed the conclusion to which we are anxious to bring our readers, in the following record of his experience:

“The extreme difference of opinion and the opposite measures recommended by practitioners, arise chiefly, I am satisfied, from their treating of every variety of puerperal fever as one and the same disease; whereas there is perhaps not any other which exhibits a greater diversity of character in different situations, and even in the same situation, at different periods. In some the fever is accompanied by symptoms indicative of the most active inflammation, such as to forbid the least delay in the free use of venesection, and the decided employment of anti-phlogistic measures. This form of disease, *which is by far the most manageable, is generally met with in private practice.* Puerperal fever, when epidemic in hospital, is directly the reverse; at least, in *four* epidemics which I have witnessed, the symptoms were usually of the lowest typhoid description, the pulse being so feeble and indistinct, as to make you dread in many even the application of leeches; the patients, in several instances of this form of the disease, exhibiting somewhat the appearance of those labouring under cholera.”

Dr. Copland states—

“That a most rapidly fatal and most malignant form of puerperal fever is occasionally developed in lying-in hospitals, which is certainly not characterized by uterine phlebitis, nor by purulent collections in the uterus or its appendages, nor even in some cases by peritonitis; the chief lesions often being merely a remarkable alteration of the blood, general lacerability of the tissues, or loss of their vital cohesion soon after death, with a dirty, muddy, offensive, and sometimes a scanty effusion into the serous cavities.”

The last evidence we shall adduce is of the more value from its recent occurrence. Since December, 1854, an epidemic of puerperal fever has prevailed more or less in Dublin and its neighbourhood. An account of this epidemic, as it appeared in the Lying-in Hospital, was read by the present master of the hospital, Dr. M'Clintock, before the Obstetrical Society, last March, from which we have been permitted to lay before the reader the following extracts. After stating the difficulty he felt in classifying the cases, Dr. M'Clintock proceeds:

“There were two features, however, common to them all—viz., a very rapid circulation, the pulse ranging from 120 to 140; and a marked adynamic type, so marked, indeed, that in two cases only did I feel justified in making trial of phlebotomy, and these, as you may suppose, were selected cases; yet in each of them, the supervention of syncope rendered it necessary to discontinue the bleeding—one losing about seven, the other nine fluid ounces; and what is still more worthy of attention, is the fact, that in neither of these instances did the blood exhibit, after some hours' standing, any of the characters indicative of inflammation.

“I think I am justified in asserting, that the prevailing character of the tongue in the late epidemic was a close approximation to what is usually called the ‘typhoid tongue,’ and this is one symptom wherein it differed from the epidemic of 1845, in which the tongue presented most usually a broad, soft, creamy appearance.

“There seemed to exist throughout the epidemic a strong tendency to putrescence or sloughing of the uterus and vagina, and this, too, quite irrespective of the length or character of the labour. In six cases we had direct proof of the existence of this gangrenous condition; two of these were patients who had sloughing of the vagina, and recovered.

“It has been already stated, that in every instance the pulse was found to be very rapid. At the commencement of an attack it was rarely below 112, occasionally much higher; and as the symptoms became more developed, and the

disease made progress, the pulse commonly rose to 130 or 140. The other characters of the pulse were sufficiently marked to render them deserving of notice. In no one instance could we have applied to it the epithet 'incompressible.' On the contrary, it was invariably soft and yielding, and gave to the finger a sensation that is best described by calling it liquid or undulating.

"I believe it may with truth be affirmed, that bleeding in this epidemic was inadmissible. The only cases in which it was tried proved it so, and both of them died; the disease seeming to be wholly unaffected, if not aggravated, by the measure."

After some explanation of the difference between his experience and that of those who have found bleeding efficacious, Dr. M'Clintock continues—

"In support of the supposition here thrown out, I would beg to draw attention to the significant fact, that each of these four authors, Gordon, Hey, Armstrong, and Meigs, the great champions for the lancet in puerperal fever, derived their experience of the disease from private practice only; and it is now well established, that no parallel as to the mode and results of treatment can be fairly instituted between the disease as it presents itself in hospital and in general practice."

Again—

"Wine was ordered to nearly all our cases, and to some from a very early period of the disorder. All the patients who recovered from a bad attack of the complaint, got wine to the extent of eight, ten, or twelve ounces in the twenty-four hours, and this from the second or third day of their illness. Some of them, too, got brandy along with the wine. In forming an estimate of the utility of stimulants, I would wish to express myself with the strictest caution and reserve; but I can with truth say, that on no occasion did I see reason to regret their exhibition, whilst in some cases their good effects did not admit of doubt. If I had to encounter another outbreak of puerperal fever similar to that first described, I should, with my present knowledge, give wine much more freely to my patients.

"After a calm and deliberate survey of the symptoms, treatment, and other attendant circumstances of the late epidemic, viewed in relation to the all-important subject of treatment, the practical conclusion at which I have arrived is embodied in this short precept—'to leech promptly, to purge actively, and to stimulate freely.'"^{*}

During the period embraced by Dr. M'Clintock's report, there were 182 women delivered in the hospital, of whom 38 (1 in 5) were attacked by the disease; 21 died and 17 recovered.

Much more evidence of the same kind might be adduced, but we have surely collected sufficient to show that there is a form of puerperal fever, very different from the form truly and graphically described by Dr. Meigs and others, unless we are prepared to deny either the powers of observation or the truthfulness of the authors who have described it; indeed, a glance at the different rate of mortality would alone be sufficient to justify this conclusion. For example, Dr. Collins lost 56 out of 88 cases, Dr. M'Clintock, 21 out of 38, whilst Dr. Meigs lost only 3 out of 13, and Dr. Rutter (quoted by him) 18 out of 70 cases. What, then, is its real character? It is not truly inflammatory, not sthenic, and the local lesions are trivial at the outset of the attack, and for hours or days afterwards; nay, in some cases there seems scarcely any local disease. For our own parts, we are constrained to believe it to be a genuine essential

* Dublin Quarterly Journal, May, 1855.

fever, as much so as typhus, occurring after delivery, and accompanied, though not necessarily, by local affections, naturally to be expected under the circumstances. There are several considerations which seem to confirm this view.

1. It exhibits a very close resemblance to typhus fever in its general type, its symptoms, its course, many of its complications, and in its treatment, as has been observed by several authors. This analogy has been the subject of a paper (unpublished) by Dr. H. Kennedy, read before the Dublin Obstetrical Society, Jan. 1849, from which I am permitted to make the following extract:

“If we leave out,” he remarks, “Dr. Fergusson’s first division of puerperal fever, we shall find that the remaining three are exactly represented in typhus fever. The ‘gastro-enteric’ form of puerperal fever has, as far as I have seen, an exact analogue in typhus, and the same may be said of the ‘nervous;’ and should we include a ‘cerebral’ form, who does not know that it is to be met with in puerperal fever, and may so simulate typhus as to render it next to impossible to distinguish the one from the other, taking only the existing symptoms into account? The ‘complicated form,’ which is Dr. Fergusson’s fourth division, what is it, in point of fact, but what any one in the habit of seeing typhus fever must recognise at once—that is, a fever which is attended or followed by secondary inflammations, coming on usually with great rapidity, and consisting of effusions of serum, lymph, blood, or pus into any or all parts of the frame? If the morbid appearances which the uterus presents be excepted, every other peculiarity has come under my notice as a complication of typhus fever, including peritonitis, and all the varied forms of what is known as diffuse inflammation. I have repeatedly also seen the affection known as phlegmasia dolens, coming on after fever, and in men. As to the general features of the two affections, they are in all respects analogous—that is, the types of the fever which one presents are to be seen in the other.”

Let it be remembered also, that it often happens that the two diseases prevail epidemically at the same time, that to a certain extent they are promoted by the same causes, and are under similar atmospheric or climatic influences.

2. The analogy between puerperal fever and erysipelas is so close that by many the former is regarded merely as an erysipelatous affection of internal structures. They are influenced by the same climatic causes, occur epidemically at the same time, exhibit the same type of fever, and may co-exist in the same patients.

3. But we may go a step further, for we possess evidence to show that either of these diseases may excite puerperal fever in women in childbed. Take the following statement by Dr. Collins, as regards typhus fever.

“When Dr. Labatt was master of the hospital, puerperal fever commenced its attack on one occasion in the following striking manner. A patient was admitted at a late hour at night into one of the wards, labouring under a bad form of typhus fever, with petechial spots over her body; when observed next morning, she was removed into a separate apartment, where she died shortly after. The two females who occupied the beds adjoining hers while she remained in the large ward, were attacked with puerperal fever, and died. In October, 1827, when I was resident as master, an occurrence precisely similar took place. A patient in typhus fever was admitted at night into one of the labour wards, where she remained for some hours; the ward contained four beds. The three women occupying the other beds were attacked with puerperal fever, of whom two died.”

Dr. Gordon lately related the case of a woman who remained in the Hardwick Fever Hospital for some time after recovery from a slight attack of fever. She became ill again, with symptoms of puerperal fever, was prematurely confined, and shortly died of that disease. These cases appear to us decisive of the power of typhus to excite puerperal fever.

The evidence on record showing that the contagion of erysipelas can excite puerperal fever is equally, if not more, conclusive, as Dr. Copland has shown in his valuable dictionary.

“Dr. Holmes (an American author) notices, in his instructive memoir, that Dr. S. Jackson went from a case of gangrenous erysipelas, which he had been dressing, to the first of a series of cases which took place in his practice.

“Another physician, who had a series of five successive cases of puerperal fever, states that for two weeks previously to the first case of puerperal fever he had been attending a severe case of erysipelas.”

Dr. Ramsbotham also mentions several cases, and Dr. Copland concludes

“That the evidence is altogether satisfactory, that some of the series of the more malignant states of puerperal fever, have been produced by an infection originating in the effluvia proceeding from erysipelas, or by the contagion of the matter or contaminating material produced by erysipelas.”

We have received permission to quote the following cases by the medical gentleman who attended them. This gentleman was in attendance upon a bad case of erysipelas, and had just dressed the part affected, when he was summoned to a midwifery case in the country, without being allowed to return home to change his clothes. The lady had been previously in good health, had a favourable labour, but on the second day she was attacked by puerperal fever, of which she died.

Another lady, whose labour was tedious and difficult, was delivered by the forceps, and on the seventh day was attacked by severe erysipelas of the neck, back, and legs, of which she died, but without abdominal tenderness, or any other symptom of puerperal fever. Previous to her death, the nurse, who was also a midwife, was sent for to attend another lady in her confinement. On the second day after delivery she was attacked by genuine puerperal fever, of which she died.

Dr. Ramsbotham has shown, further, that erysipelas may be the result of exposure to the contagion of puerperal fever in the nurses, as in the following example:

“In 1841, when erysipelas was prevalent in Rotherhithe, a medical friend had six cases, and whilst attending these he delivered a lady, who was speedily seized with puerperal fever, and very soon afterwards died. Her nurse was attacked by erysipelas of the hand, and was attended by another surgeon. One day, after having made an incision and dressed the wound, this latter surgeon was called to a case of midwifery: puerperal fever supervened, and the patient sunk very rapidly.”

Now, here we have puerperal fever produced by the contagion of typhus fever and of erysipelas, but we can scarcely infer from that its identity with both these diseases; or if so, we must go a step further, and admit the identity of all three diseases. What element, then, is common to all three, for this may lead to a solution of the problem. It can neither be the local affection, nor the mode in which the exciting cause

is applied. The most striking characteristic common to all is the low fever of a malignant type; the fever is certainly accompanied by local affections, but these are very different in their seat, however much they may resemble each other pathologically.

4. There is good reason for believing, and this opinion is held by Rokitansky and Semelweiss among others, that puerperal fever may be excited by morbid matter adhering to the hands of those pupils who leave the dissecting-room to attend midwifery cases, and that irrespective of the disease of which the subject has died. We know, also, that puerperal fever has been apparently caused by those who attended cases after making a post-mortem examination of women who have died of the disease. Thus, in addition to general causes communicated by way of infection, we have local causes transmitted by contact internally, and to be satisfactory, our explanation should embrace all.

Now, if puerperal fever has this close relation to typhus and erysipelas, so that it is capable of being produced by either; if the primary character of the fever is the characteristic common to all; and if the state of the blood in typhus and erysipelas be the most essential pathological change it is fair to conclude that this is shared also by puerperal fever. But some similar change in the blood is the effect of poisoned wounds, and it is not unreasonable to suppose that morbid animal poison, applied by the fingers of the accoucheur to the vaginal mucous membrane, will likewise act by inducing a morbid change in the blood, and, if so, we arrive at a new pathological element, common to all the cases we have mentioned—viz., that puerperal fever consists essentially in a morbid condition of the blood, whether produced by contagion or infection *ab extra*, or by absorption within the body.

Dr. Meigs sees an objection to the absorption of pus or morbid matter from the uterine surface, in the fact that the pus globules are too large to enter the minute vessels; but he forgets that the *liquor puris* may be absorbed, or that, as Dr. Bennett has suggested, the morbid poison may be gaseous, resulting from the decomposition of pus or other morbid matter.

But another question remains, what is the relation of the local lesions and their pathological character? We have adduced what appears to us very strong arguments to show the essential character of the fever, which of course involves the supposition that the local diseases are secondary, and if, in addition, we recollect what Drs. Gooch, Copland, and others have observed—that fatal cases may occur, though rarely, without local lesions, yet with all the other characters of puerperal fever—this conclusion seems almost inevitable. Dr. John Clarke, who so admirably described these affections, observes, when speaking of the low fever of childbed, that “another question arises, whether the affection of the abdomen should be considered as the primary disease and the fever symptomatic, or the fever the disease and the affection of the abdomen symptomatic? I own that I am inclined to favour the last supposition.” Dr. Ferguson considers that the local diseases, “phlebitis, or peritonitis, or metro-peritonitis are only secondary effects of one cause.”

If these local affections be secondary in this peculiar form of epidemic puerperal fever—and we are concerned at present with no other—are they al

of an inflammatory character, or may some of them at least be the result of other modes of morbid action? It would perhaps be rash to speak dogmatically upon the subject, which is a very difficult one, but when we consider the nature of the secondary lesions in typhus fever in the light which recent researches have thrown upon them, it does not seem improbable that the analogy to which we have alluded between malignant puerperal fever and typhus may extend beyond the question of the primary character of the fever; and that at least some of the local changes—for example, purulent deposits, softening and putrescence of the uterus or pelvic viscera—may not always be due to inflammatory action; and certain it is that the post-mortem appearances do not contradict this assumption. As an example, we quote the following case, the details of which, with the morbid specimen, were recently submitted to the Dublin Obstetrical Society by Dr. M'Clintock. A young woman in her first labour was delivered by the vectis of a living child, after having had two well-marked convulsions, with an interval of some hours between them. The urine drawn off by the catheter during labour was highly albuminous. The pulse remained quick from the time of delivery. In twenty hours the belly was full, tympanitic, and extremely tender over the uterus, which was large; pulse 120. Twenty leeches were applied to the hypogastric region, which removed the pain and tenderness, but did not improve her condition, and she died in thirty-six hours from the time of delivery. Autopsy:—No trace of peritonitis. Kidneys in second stage of Bright's disease. Uterus large, soft, and flaccid. The whole interior surface presented a red, soft, gelatinous appearance, as if smeared with red currant jelly. There was no sign of inflammation or putrescence. The ovaries were pale, and softened to such a degree as to be almost pulpy.

On this very interesting subject we cannot resist quoting the remarks of one of the most philosophic physicians of the present day:

“Notwithstanding all that has been done,” observes Dr. Stokes, “in that branch of pathological medicine which treats of the local affections secondary to essential disease, many practitioners consider them as original inflammations, and to this is to be attributed a large portion of the errors in diagnosis and practice, so much to be deplored at home and abroad. If we inquire why it is that so many do not receive in its full extent the doctrine that a vast number of acute diseases cannot be explained by the theory of primary inflammation,—and again, that when inflammatory action does occur in them, it is of a secondary, reactive character, acting on tissues already altered by a process of a different kind, which is itself subsidiary to a general and essential condition,—we find that the reasons are manifold. The opinions to be overcome are the growth of many years, and may fairly be dated from the period when accurate anatomical investigation was applied to elucidate diseased structure. As might be expected, an almost exclusive attention was directed to those manifest changes induced by inflammation, so striking, not only in their earlier stages, but also in the successive periods of the process. And thus the doctrine of solidism, while it replaced the humoral theory, came to mean more than its name would imply; for while it referred diseases to an alteration of the solids, it taught that these changes had a common character dependent on some stage or phase of the inflammatory process. Alterations unattended with increased vascularity, tumour, or ulcerative action, were naturally overlooked, and this the more readily when no symptom of pain or disturbed action had been observed during life. Thus the facility which presented itself from the multiplication of instances of inflammatory action in all the great organs, and of connect-

ing symptoms with some stage of these changes, paved the way to the doctrine of Broussais, which referred so many diseases, both general and local, to irritation or inflammation. Naturally captivating from its being, as it were, the first fruits of so much past labour in a new direction,—specious in its application to a general theory of medicine, and supported by the bold writing and great experience of its author,—this doctrine soon became on the Continent an accepted medical creed. In the British Isles, too, though more cautiously received, it influenced the minds of students and of the younger members of the profession. . . . The unhappy division of the profession into separate corporations of medicine and surgery operated strongly in extending the adoption of an inflammatory theory of disease, for in the wards of a surgical hospital the surgical student saw little or nothing but the effects of inflammation. Thousands of practitioners were created who had never seen a case of typhus fever, and who, even when other forms of adynamic diseases presented themselves, were misled by the specious terms of diffuse or erysipelatous inflammation. It is difficult to change the opinions in which we have been brought up, and it often happens, especially in medicine, that when an opportunity is given of extended experience, the mind, from age, habit, or indolence, has become unfit to profit by that opportunity. When we reflect on this, and that for many years a large proportion of British practitioners have received an education almost wholly surgical, and have entered on practice with but scanty knowledge of the non-surgical and the essential diseases in this country, and still less of those met with over the wide surface of the British dominions, we can understand how extensive must have been the application of antiphlogistic treatment to disease which was itself antiphlogistic.”*

Having examined this very interesting subject as fully as our limits will permit, we shall, in conclusion, lay before our readers a summary of the conclusions at which we have arrived.

1. That the characteristics of puerperal epidemics vary as widely and as essentially as do those of other epidemics, and may be either of a sthenic or asthenic nature; from which it follows, whilst they may be grouped together on the ground that they all occur after delivery, and involve certain organs equally, these pathological peculiarities require a different classification, and all attempts to apply the same theory and the same rules of practice to all must necessarily fail.

2. That we recognise, upon adequate testimony, and from our own experience, epidemics of puerperal fever, of which the general type was sthenic or inflammatory, and the local affections the result of inflammation: nor do we deny that the local disease may have been primary and the fever secondary, whilst the result of early and active antiphlogistic treatment was in accordance with such character of the disorder.

3. That we should be rejecting testimony of the very highest authority, and throwing aside the results of our own observations, did we not also admit that epidemics of puerperal fever do occur, especially in lying-in hospitals, of quite an opposite character, in which the general disease is of an adynamic type, presenting typhoid symptoms, and in which the local lesions exhibit a subordinate and secondary character.

4. That judging from analogy, from the mode of production and extension, and from certain peculiar exciting causes, it appears highly probable that the essential pathology of the “typhoid,” “malignant,” or “low childbed fever,” consists in a morbid condition of the blood, whether induced by morbid influences applied by the way of contagion or infec-

* Stokes on the Diseases of the Heart and Aorta, p. 436; note.

tion, or by the absorption of morbid matter, either fluid or aëriform, within the body.

5. It seems most probable that the local lesions in the typhoid epidemics of puerperal fever hold an analogous position to the local affections of typhus fever and other typhoid diseases—i. e., are pathologically secondary, and the consequence of the general disease.

6. As in the local affections of typhus fever, so in those of puerperal fever, there is at least room for doubt how far they are the results of inflammatory action.

II. Let us now turn to the question of contagion—a subject of great interest and difficulty in itself, but which has been rather obscured than explained by the loose and general way in which it has been treated by medical writers, and which, by the popular view taken of it, is often the cause of great distress to the friends of the patient, and often of great injustice to the medical attendant. At present the popular opinion is, that the disease is a contagious one, *therefore* every case that occurs is attributed to the accoucheur having conveyed it from one patient to another.

Under the term contagion, or infection, is included not merely the possibility of the disease being communicated, by a patient labouring under it, to a woman recently delivered, but also its communicability by a third party not suffering from it,—as the accoucheur, or nurse. In both aspects the question is one of extreme interest, but our object now will be rather with the latter view, as one which has a practical though very painful bearing on the profession. We are met at the outset by the difficulty which attends every attempt to define or prove the contagious nature of any disease which is admitted to be epidemic—viz., that the latter influence will explain equally well almost all the examples adduced in favour of contagion, inasmuch as the conditions requisite for contagion or infection involve those calculated to promote the spread of an epidemic: *“viz., breathing the same air, and being exposed to the same atmospheric influences.”* Under such circumstances it is very hard to draw a marked line of separation, and moreover it is quite possible, as in the case of cholera, that an epidemic may be first lighted up by contagion, but afterwards spread independent of it. The question has divided the highest authorities in obstetric literature: on the one side we find such names as Hulme, Leake, Hull, Baudelocque, Tonnellé, Dugès, Jacquemier, Kiwisch, Dewees, &c., denying its contagiousness; and on the other, Gordon, Clarke, Denman, Burns, Hamilton, Blundell, Gooch, Ramsbotham, Locock, Douglas, Rigby, Channing, Holmes, Copland, &c., affirming it.

We would venture to suggest, that possibly some explanation may be found of this contrariety of opinion, in the different character of different epidemics, to which we have so often alluded. May it not be that whilst the typhoid or malignant childbed fever is contagious, the sthenic or inflammatory variety may be less so, or perhaps not so at all?

It should also be kept in mind, that even if it could be demonstrated that the disease may be propagated by contagion, this is but one mode of its extension, and certainly the least influential.

Dr. Meigs ranges himself among the anti-contagionists, and charges à

la Zouave at all who differ from him. His arguments, however, have rather the appearance of being the consequence of a preconceived opinion as to the nature of the disease, than the result of an impartial examination of evidence. Thus, he observes:

“If, as some pretend, childbed fever is a fever indeed, and not a mere topical inflammation, that gives rise to febrile phenomena by means of the irritating power of the local disease, there might remain some chance of a probability that such an affection should be a contagious one. I know not what ideas you may have imbibed upon these points, but it is the object of this work to prove that childbed fever is a simple state of inflammation in certain tissues of pregnant women, and of women lately confined, and that the fever that attends it is a natural effect of intense constitutional irritation from the local disorder. So far as I have been able to investigate the subject by reading and by clinical observation, as well as by microscopic researches, I rest deeply convinced that the fever does not take the initiative, except in very rare instances; but on the contrary, that an area of inflammation being first established, the reactions issue thereupon: and I beg you to observe, that in all truly contagious disorders the constitutional affection leads the train, and brings on the topical lesions after an indispensable period of incubation.” (p. 57.)

Now, if there be any truth in the conclusions stated in the former part of this review—viz., that in certain cases, and in certain epidemics, childbed fever is an essential disease, it follows, according to Dr. Meigs, that it may in such instances be contagious; we do not contend for more than this, and the reader will decide for himself how far we have shown grounds for this conclusion.

Dr. Meigs' next argument is, that contagious diseases are always specific, and can only give rise to similar diseases, “and further, they affect the race of mankind without respect to age or sex of those who become exposed. There is no presumable exception among men.” Both these propositions are true to a great extent, especially if we confine our attention to the local affections; but to both there are many exceptions. We are by no means sure that male attendants may not take a fever from puerperal patients; one fatal case came under our observation, in which a medical student, in attendance at the lying-in hospital, was attacked by fever exhibiting all the general characteristics of the epidemic which then prevailed; though if Dr. Meigs insists upon the presence of the local affections to constitute similarity of disease, we must admit that they were absent!

Even Dr. Meigs admits that pregnant women may take the disease, and we have also seen that nurses may be attacked by erysipelas when attending upon puerperal fever.

Again, Dr. Meigs observes that “it is an absolute condition of contagion, that it must undergo some certain incubation. If it is a ferment, it must have time to ferment; if a spore, it must have time to develop spores; if infusorials, or infusorial ova, there must be time for incubation. Such is the law of variola, vaccinia, measles, varicella, &c.” And then he quotes a case from Tonnellé, in which death took place in twenty hours, and another from Kiwisch, fatal in eight hours.

Many a woman has entered upon her labour in apparent health, and has scarcely given birth to her offspring before she is attacked by metrophlebitis, and then been deprived of existence within less than twenty-four hours. Forty-eight

is frequently time enough to effect the dissolution. Are these inoculations? Are these contagious? Are these infections *ad distans*? It is nonsense to say so." (p. 90.)

Of course, if Dr. Meigs could fix definitely the period necessary for incubation, his argument would be conclusive against those cases in which no such period elapsed. But he must know that in the majority of cases the disease does not show itself till the third or subsequent days, which does allow an interval for incubation, so far as we know, long enough. However, we are far from contending that the disease is chiefly propagated by contagion, although we think that sufficient evidence has been adduced to show the probability of this being one mode by which it is extended. We believe that its spread is far more due to its epidemic character, although we agree with Professor Dubois that it is almost impossible to say how much is due to contagion, and how much to epidemic influence, though few who have had much experience in hospitals will be inclined to deny the influence of contagion altogether.

Leaving the general question of contagion from one patient to another, let us for a short space examine into the evidence in favour of the communicability of the disease by a third party from a patient labouring under it to another during or after her delivery. The exact value of the facts on record will be better estimated by a little classification.

1. It seems impossible to doubt that contagious matter capable of exciting puerperal fever may be conveyed by a third party unaffected by it; for example, in the cases already mentioned of puerperal fever following the services of medical men and nurses who were in attendance upon erysipelas immediately before. The cases are too remarkable and too numerous to be regarded as coincidences, nor would even the prevalence of an epidemic of puerperal fever at the time invalidate our conclusions; it might certainly render the cause more influential.

2. It is the recorded opinion of Rokitansky and Semelweiss that morbid matter acquired in the dissection of subjects not dying of childbed fever may be conveyed by the dissector and excite the disease in a patient delivered by him; and to this, among other causes, has been attributed the prevalence of puerperal fever in the wards of the Vienna Lying-in Hospital. A celebrated foreign practitioner attributed two outbreaks of this disease among his private patients to his having handled morbid specimens just before attending the patient in her accouchement.

3. We should, therefore, have less difficulty in believing that similar effects may be produced by those passing from the dissection of puerperal patients to the delivery of healthy ones, especially if the most rigorous precautions were not observed. For instance, in the autumn of 1821 Dr. Campbell, of Edinburgh, attended the dissection of a married woman who died of the disease, after an abortion of the early months; he removed the pelvic viscera and external parts, and carried the whole in his coat pocket to the class room. The next morning, dressed in the same clothes, he assisted with some of his pupils at an instrumental delivery at Bridewell. This woman was seized with the same affection, and died. The same night he accompanied Dr. Orr to the delivery of a woman residing in the north back of the Canongate; she was equally unfortunate; and three other poor women shared the same fate in quick succession. In a

subsequent year, 1823, he assisted at the dissection of a childbed fever case, but could not wash his hands with the care he desired; thence he went to attend two other women in labour, both of whom died.

At a meeting of the College of Physicians, Philadelphia, U.S., Dr. Warrington stated that after assisting at an autopsy of puerperal peritonitis he was called upon to deliver three women in rapid succession. All these women were attacked with different forms of what is commonly called puerperal fever.

“Mr. Davies states that in the autumn of 1822 he met with twelve cases, while his medical friends in the neighbourhood did not meet with any, or, at least, with very few. He could attribute this to no other cause than his having been present at the examination of two cases, and his having conveyed the infection to his patients, notwithstanding every precaution.”

“A young surgeon shortly after examining the body of a sporadic case that had died, delivered three women, who all died of puerperal fever.”

“Mr. Ingleby states that two gentlemen, after the post-mortem examination of a case of this disease, went in the same dress, each respectively, to a case of midwifery. The one case was attacked in thirty hours afterwards, the other in three days. One of the same surgeons attended, in the same clothes, another female, and she was attacked on the evening of the fifth day, and afterwards died.”*

Now with regard to the cases attended immediately after the post-mortem dissection, there seems little room for doubt as to the exciting cause of the fever. It may have been conveyed in the clothes or on the hands of the accoucheur, but it is, at any rate, adequate to the effect, and the sequence is too simple and too close to be rejected.

4. Can we venture to say the same of the following case:

“Dr. Merriman mentions in the ‘Lancet’ for May 2, 1840, that he was present at the examination of a case of puerperal fever at two P.M. *He took care not to touch the body.* At nine o’clock the same evening he attended a woman in labour; she was so nearly delivered that he had scarcely anything to do. The next morning she had rigors, and died in forty-eight hours.”

We do not know whether puerperal fever was epidemic at the time, but the cause suggested seems so inadequate that we should be inclined to look for some other explanation.

5. So far, then, we have seen medical men engaged in handling morbid matter, their dress and persons exposed to the effluvia from dead bodies, and passing directly to attendance upon lying-in women; here we have a distinct appreciable exciting cause, adequate to the production of disease in healthy persons, and which may have been, and probably was, conveyed to the patients who were first attended, and in whom puerperal fever appeared. But in several instances the disease was not confined to the first women attended, but appeared in others delivered successively. How are we to explain this, and how can we explain the pertinacity with which puerperal fever seems occasionally to track the footsteps of one or two practitioners, whether at first lighted up by morbid matter derived from dissection or not? Take the following examples. Dr. Gooch mentions that—

“A general practitioner, in large midwifery practice, lost so many patients from puerperal fever, that he determined to deliver no more for some time, but that his partner should attend in his place. This plan was pursued for one month, during

* Copland’s Dictionary. Art., Puerperal Fever.

which not a case of the disease occurred in their practice. The elder practitioner being then sufficiently recovered, returned to his practice, but the first patient he attended was attacked by the disease and died.*"

This latter fact seems to us to prove that the disease was epidemic at the time. Similar instances have come to our own knowledge more recently.

"Dr. West, of Philadelphia, states that seven females delivered by Dr. S. Jackson, in rapid succession, were all attacked with puerperal fever, and five of them died. These were the only cases that occurred in that district, for the women became alarmed, and sent for other assistance."

"A physician in Boston, U.S., had the following consecutive cases:—On March 24th, April 9th, 10th, 11th, 27th, and 28th, and May 8th, seven in all, of which five died. He then left town."†

Another physician writes to Dr. Holmes as follows:

"The first case was in February, 1830, during a very cold time. She was confined on the 4th, and died on the 12th. Between the 10th and 28th of this month I attended six women in labour, all of whom did well except the last, as also two who were confined March 1st and 5th. Mrs. E., confined February 28th, sickened and died March 8th. The next day, March 9th, I inspected the body, and the night after attended a lady, Mrs. G., who sickened and died on the 16th. The 10th I attended another, Mrs. B., who sickened but recovered. March 16th I went from Mrs. B.'s room to attend a Mrs. H., who sickened and died on the 21st. The 17th I inspected Mrs. G. On the 19th I went directly from Mrs. H.'s room to attend another lady, Mrs. G., who also sickened and died on the 22nd. While Mrs. B. was sick on the 15th, I went directly from her room, a few rods, and attended another woman, who was not sick. Up to the 20th of the month I wore the same clothes. I now refused to attend any labour, and did not until April 21st, when, having thoroughly cleansed myself, I resumed my practice, and had no more puerperal fever. The cases were not confined to a narrow space. The two nearest were half a mile from each other, and half that distance from my residence. The others were from two to three miles apart. There were no other cases in their immediate vicinity."‡

Dr. Ramsbotham has known the disease to spread through a particular district, or to be confined to the practice of a particular person, almost every patient being attacked by it; whilst other practitioners had not a single case; and he considers the distemper as being capable of conveyance not only in common modes, but through the dress of the attendants on the patients.

In Sunderland, 40 out of 53 cases occurred in the practice of one surgeon and his assistant.

Dr. Robertson, of Manchester, states, that between the 3rd of December, 1830, and January 4th, 1831, a midwife attended 30 patients of a public charity, 16 of whom had puerperal fever, and all died. Other midwives of the same institution attended 380 women during the same time, and none suffered from it. He also mentions the case of a practitioner, who introduced the catheter for a poor woman in puerperal fever, late one evening, and attended a lady in her confinement during the same night, who was attacked with puerperal fever on the second day.

Analogous cases have been recorded by Dr. Pierson, of Salem, U.S., Dr. Peddie, and Mr. Beecroft; and such examples are, doubtless, very startling, and require a careful examination, to ascertain their exact value as bearing on the question at issue; but we shall first hear what Dr.

* On the more important Diseases of Women.

† Copland's Dictionary.

‡ Ibid.

Meigs says on the subject. His first argument is from personal experience.

"I have practised midwifery for many long years. I have attended some thousands of women in labour, and passed through repeated epidemics of childbed fever, both in town and hospital. After all this experience, however, I do not, upon careful reflection and self-examination, find the least reason to suppose that I have ever conveyed the disease from place to place in any single instance. Yet for many years I carefully considered whether such a transfer by a third person might be possible, and carefully read the statements of various authors to that effect. In the course of my professional life, I have made many necroscopic researches of childbed fever, but never did suspend my ministry as accoucheur on that account. Still I certainly never was the medium of its transmission. I have, in numerous instances, gone from the bedside of women dying of childbed fever, whether sporadic, or the most malignant degree epidemic, without making my patients sick. I have also endeavoured to assist my brethren, when they had such cases and I had none.

"In a series of labours, 468 in number, and beginning with No. 1, I find that Nos. 18 and 19 were affected, and that No. 18 died with childbed fever; No. 31 was sick, but recovered; Nos. 195 and 259 were sick, but recovered; but 291 died, as did also 293. Nos. 332, 339, 435, 444, and 445 were attacked and recovered. The above cases—viz., 18, 19, 31, 195, 259, 291, 293, 332, 339, 435, 444, 445, 455, are, in all, 13 cases in 468 labours, of which 3 died and 10 recovered. Now, if I was the medium of contagion for any one of that series of 468 confinements, why did I poison them in the ratio and order above set forth; and why did I not communicate the disease in more than 13 out of 468 cases? What became of my nebula from 31 to 195; to 259, and between 291 and 445, and so to the end, or 468? Such a table is far more easily explained by regarding the falling-out of the cases as coincidences and accidents, than as material causations, through a private pestilence." (p. 102.)

Again, as regards the singular limitation of the disease to the practice of one person, Dr. Meigs observes:

"At page 631 of my work (2d ed.) I say—

them being more than two miles from the others. At that time many women were attacked, in various parts of Philadelphia, as well as in the State of Pennsylvania; yet so far as has come to my knowledge, no other medical gentleman happened to encounter such a great number of childbed fevers as he did. I visited, in consultation with him, some of the very worst of the cases, and touched the patients, and was as liable to imbibe or to be clothed with the effluvia from their bodies as he was; nevertheless, I did not carry poison or other cause of disease to any patient of mine; and if not I, then how should he become capable of doing so? He is a gentleman who is scrupulously careful of his personal appearance, of great experience as a practitioner, and well informed as to modern opinions on the contagion of childbed fever. Still those of you who are contagionists will say that he carried the poison from house to house, and if so, then you ought to give some *rationale* of the fact. Did he carry it on his hands? But a gentleman's hands are clean. Did he carry a nebula or halo about him? Then why not I also? If the nebula adhered to his clothing, it might as well have adhered to mine.

"What will you say, young gentlemen, of the experience of my friend, Dr. D. Ratter, formerly of Philadelphia, but now of the city of Chicago, who passed through terrible scenes here, in an epidemic of childbed fever, some years ago, when he had a most extensive midwifery practice in town and country? During that sad time, I saw several fatal cases with him in consultation; and though he

seemed to be tracked by the cause of the disease, to judge by the numerous attacks of it in his lying-in patients, I was not tracked by it. I took no precaution, except such as every decent man should be supposed always to take; yet I never did carry the disease from his cases to any houses where I visited lying-in women. But he was charged with being a carrier of contagion. How could he carry the cause? What was the cause? Was it some ozone that stuck to his hands or coat? Was it a nebula, a halo, or a miasm that mixed with the hairs of his head or the woollen or cotton fibres of his dress? or an exhalation from his skin, or a halitus from his lungs, like the fiery breath of Cacus? And can you say of him, as Virgil sings—

“Faucibus ingentem primum, mirabile dictu
Evonit.”—*Æneid*, lib. viii. p. 252.

Come now, was not such a poison more sticky than bird-lime, seeing that Dr. Rutter, worn out with fatigue, and wounded in spirit by his cares for the unfortunate victims of an epidemic disease, left the city for the purpose of gaining some strength, and to escape from the repetition of such disheartening labours, and that even a quarantine could not liberate him from this poisoned cloud? One might hope it would have been blown away by the wind, or that it would have evaporated or become too dilute to kill, after a ride of seventy miles, and an absence of ten days. But it happened, after this rustication of ten days, at a distance of thirty-five miles from the city, that your bird-lime or cloud still adhered to him, as your contagionists would say. And more than that, he could not even wash it away or shave it off; for upon coming back to the city, and to his professional toil, before he engaged in practice again he caused his head to be close shaved; he entered a warm bath and washed himself clean; he procured a *new wig, new clothes, new hat, new gloves, and new boots*. He did not *touch* anything he had worn, and took the precaution to leave his pencil at home, and his watch. Well, what do you think happened next? He went out to attend a lady in labour, who had a favourable parturition, yet was next day assailed by a horrible childbed fever, of which she died, in spite of all his efforts, and mine to help him; for he called me in consultation immediately after being summoned himself to her chamber. I know that that lady died with peritonitis. I was a great deal with her in her illness, but she did not poison me or my clothes; for although I went on with my practice, I poisoned nobody, and made nobody have even so much as a finger-ache.

cases “Dr. Rutter repeated this attempt at personal disinfection at a subsequent period, which was two years later, and with the same ill-success. The gentleman was much and disparagingly spoken of on account of the above-mentioned events in his practice, which I cannot but regard as both cruel and unjust, particularly as his success in the treatment was most brilliant; for during the epidemic he had charge of 70 cases, of which he lost only 18, and I know not the man who can boast of a higher triumph of his art of healing in this malady.” (p. 102.).

Let us now look a little closer into this matter. The broad fact apparently established by the foregoing observations is, that puerperal fever does sometimes prevail chiefly, or is altogether limited, to the patients of certain practitioners, and the question arises, To what is this owing? The question is *not*, whether contagion is the only, or the chief, or the ordinary means by which the disease is propagated; for it is admitted on all hands to prevail epidemically. Nor is it the question, whether, under favouring circumstances, contagion may not be conveyed to the patient by the accoucheur, for we have related cases in which it seems impossible to doubt that this took place. We must therefore eliminate from the foregoing examples the case of the physician who wrote to Dr. Holmès, because, having made post-mortem examinations, his experience may rather be referred to Section 3. In most of the instances, we are not

told whether the practitioner examined the bodies after death; if they did, we cannot deny that there was a possibility that they might have carried the infection.

Again, if, as Dr. Rigby remarks, "the discharges from a patient in puerperal fever are highly contagious," it is at least possible that the case of the midwife mentioned by Dr. Robertson may be thus explained, inasmuch as her duties about her patients would necessitate more or less contact with the excretions.

Excluding these classes of cases, evidence enough remains to show that the fever does sometimes follow in the track of particular accoucheurs; and the real question before us is, whether it does so by contagion conveyed by him from other patients, in spite of the ordinary precautions, or in certain cases, notwithstanding the extraordinary precautions of baths, change of air, change of clothes, &c., or whether in such cases the prevalence of an epidemic of puerperal fever is a sufficient explanation, admitting it to exhibit caprices similar to other epidemics. It is impossible to bring the matter to a demonstration either way, difficulties meet us upon either supposition, and perhaps the best plan to adopt will be for us to weigh these difficulties separately.

Against the explanation which attributes, with Dr. Meigs, all to epidemic influence, is the fact of its greater prevalence in the practice of certain medical men, and its being in some cases apparently limited to them. That one man should see more cases than another of any epidemic disorder is common enough, and would be no difficulty in the present case; but that one should see all, and others none, does seem rather startling. But is the proof of the latter sufficiently conclusive and sufficiently extensive? Dr. Gooch does not tell us whether the disease was epidemic or not, nor does Dr. West. The gentleman who wrote to Dr. Holmes states that no other cases occurred in the vicinity, but we have rejected his example as being one of *possible* contagion on the ground of his post-mortem examinations. In Sunderland there were at least thirteen cases which occurred in the practice of others, besides the surgeon and his assistant. The two examples related by Dr. Meigs occurred during epidemics. So that it must be confessed that the evidence we possess to show the insufficiency of epidemic influence as an explanation, and the necessity of finding some other cause for its greater prevalence in a particular direction, is neither extensive nor positive.

The explanation which attributes this peculiarity to contagion has the merit of being simple and apparently adequate, but the difficulties on examination are more numerous and fully as great. Assuming for a moment that the disease can be only communicated during labour, let us recall to our readers what takes place during an ordinary visit to a patient in puerperal fever, during which time the infection is to be taken. The visit may occupy five or ten minutes, the physician stands by the bed, feels the pulse, examines the abdomen, but does not come in contact with the discharges. Having made his investigations, he washes his hands carefully, and then pays more visits, passing through the air, until evening, or until he is called to a labour. If many hours elapse, he must have washed his hands several times. Yet, in spite of all this, we are to suppose that he carries morbid matter on his hands; or clothes, acquired

from the fever patient, enough to poison the lying-in woman. And not only this, but the explanation is supposed to be equally valid even though he change his clothes, thus limiting the infection to the hands, and even though he use chloride of lime or potash.

If the "morbid matter be conveyed on the hands, the infection, we suppose, and such seems to be the general opinion, must be imparted during labour; but if on the person or clothes, the effect might of course be produced subsequently, and hence another difficulty. During the visit, the consulting physician is as close to the fever patient, examines her, handles her quite as much at the visit as her ordinary attendant, and, it may be assumed, adopts afterwards much the same precautions. Yet we do not hear of his conveying the fever to his own patients in any case, and we have Dr. Meigs' positive statement that such an occurrence never took place in his practice. The advocates of contagion should explain this.

Again, in all contagious diseases, the intensity of the contagion imparted to, and conveyed by, a healthy person (as in scarlatina, for example) must surely be in proportion to the shortness of the time which elapses between his visit to the sick person and to the party to whom he conveys it: in other words, that his chance of so conveying it would diminish with the lapse of time. For example, an accoucheur visits a patient in puerperal fever, suppose, and acquires this contagious property; if this rule be true, the first patient he attends will be more liable to take the disease than the second, and the second than the third. How then, explain the fact on the principle of contagion, that no such sequence of attacks is observed? The cases affected observe no such order, as the reader will see by turning back to Dr. Meigs' registry.

Moreover, in two of the most striking cases we have quoted, Dr. Gooch's and Dr. Rutter's, there is a circumstance which is not reconcilable with, or explicable by, the doctrine of contagion, as we understand it. In the one case a month, and in the other ten days, of absence elapsed, and the latter was accompanied by a complete renewal of clothing, and yet the first case attended by both was attacked by puerperal fever. Are we to attribute this to remaining contagion, and if not, does it not point directly to some other influence which may have operated previously as well?

Thus, a belief in the contagiousness of puerperal fever under ordinary circumstances, and excluding the cases in Sections 1, 2, and 3, must involve, on the one hand, the conclusion that it is of all contagious disorders the most virulently contagious, inasmuch as it assumes that it can be conveyed by a healthy person exposed for a few moments only to its influence, to a third party hitherto in health, and this notwithstanding that the hands, the only part in contact with the sick person, have been carefully washed, the clothes changed, and the entire person exposed to the air, it may be for hours; and yet, on the other hand, that this contagious property limits itself to the ordinary attendant, and does not affect the consulting physician. Admitting that we cannot fully and satisfactorily explain the limitation of the disease on the supposition of epidemic influence only, we ask our readers whether the difficulties attendant upon the explanation by contagion are not more insuperable?

In conclusion, therefore, whilst we feel compelled by the evidence on record to admit the possibility of puerperal fever being conveyed and communicated or excited by those who attend midwifery cases after being employed in dissection or post-mortem examinations, and also by those who are much in contact with the patient or the discharges, especially if strict precautions are not adopted as to cleanliness and change of dress, we do not feel that in other cases, where no such conditions exist, the evidence at all justifies our attributing the spread of the disease to contagion, and we think fewer difficulties and contradictions are incurred by attributing its extension to epidemic influence, and its limitation to conditions or circumstances of which we are at present ignorant.

III. *Treatment.*—We have occupied so much space with the consideration of the two first portions of our subject, that our observations upon the treatment must be very brief indeed. If we have succeeded in showing that certain epidemics, have a typhoid character, it follows as a necessary consequence that the treatment which is successful with cases of a sthenic inflammatory type, will be quite unsuitable for them. In the epidemics witnessed by Dr. Meigs, he found early and ample blood-letting the principal, almost the sole remedy :

“I hope, my dear young gentlemen, you will rely, then, upon venescence as the most effectual, and indeed only trustworthy power vouchsafed to you in these trying circumstances; and I pray you, in executing this delicate task, to observe the following methods:—Let the woman be raised in bed, upon pillows that may support the trunk in a highly inclined position, though not in an absolutely upright one, unless she be still possessed of considerable strength. Select the most proper, which is the largest and most turgid vein in the arm, and open it by a free incision, to produce a copious jet. A clear, bright light should invariably fall on the patient's countenance, to enable you to judge, by its hues and psychical expression, of the influence of your operation during the time while it is in progress.

“The design of the operation, in childbed fevers, should never be merely to fulfil some purpose of presumed expediency, to lessen a little some pain, to diminish a somewhat troublesome cephalalgia or a vexing heat,—it is to be done with a view *jugulare febrem*, by impressing upon the motive powers of the circulation such an overmastering influence that the inflamed tissues may afterwards successfully resist the overbearing power of the general vascular reaction, which was first aroused by them alone.

“Leave off with eight, twelve, or sixteen ounces abstracted, and you may go away from the bedside, saying, ‘She will surely die;’ but if you will courageously persevere until twenty-four or more ounces, not too many more, are taken away, you may retire after your ministration, feeling assured that the duty is well done, and believing that the life of the patient will be saved.” (p. 256.)

We do not doubt the evidence of Dr. Gordon or Dr. Meigs as to the results of this method in the cases they saw, but we contend that in other epidemics it would simply hasten the death of the patient: and of late years, and in these countries, this typhoid character has chiefly characterized the disease. Contrast Dr. Meigs' directions and views with the opinions of Dr. Collins, as they are stated in a letter to Dr. Meigs, and published in his treatise ‘On Obstetrics:’

“At pages 609-10, you compare the mortality in puerperal fever under my treatment, and that of my distinguished friend, Dr. Robert Lee; of London; to prove

the greater success when general bleeding has been more frequently adopted by him. The great and markedly distinguished feature between Dr. Lec's cases and mine has, however, been overlooked; as mine were all *hospital patients*, whereas his were treated *at their own dwellings*. This was also the case with the late Dr. Gordon's patients, to whom you so deservedly allude. The disease with us, and I believe universally, is as different in hospital and out of hospital as it is possible to imagine. Please look to my observations, pages 390-1-2, &c., where I have stated the patient to be little more than a shadow, and to exhibit the appearance of those labouring under cholera, so as to make us dread even the application of leeches. The fever is of the lowest typhoid character, with the pulse so feeble and indistinct as to totally prohibit general depletion. This form of the disease is singularly intractable, and truly-fatal; whereas the inflammatory form of puerperal fever, such as is usually met with out of hospital, may be treated with considerable success.

"I should have stated, that few physicians have witnessed the results of general bleeding to a greater extent than I have done, as the master of the hospital who preceded me, and to whom I was assistant, was a strong advocate for it; but the mortality was so frightful he was forced to abandon it. He bled instantly and copiously, but with the most fatal results. Such is the character of almost all our hospital epidemics."

The experience of the present master, Dr. M'Clintock, we have already seen, is precisely similar in the recent epidemic; and we have only to add, that of late years, so far as our own limited experience enables us to judge, the same typhoid type and intolerance of general bleeding characterizes, more or less, the cases met with in private practice; following in this the change of type, and consequent change of treatment, necessary in fevers and febrile diseases generally.

Fleetwood Churchill.

REVIEW IV.

1. *Traité de la Syphilis des Nouveaux-Nés et des Enfants à la Mamelle.* Par P. DIDAY, ex-Chirurgien-en-chef de l'Antiquaille (Hôpital des — Vénériens de Lyon).—Paris, 1854. pp. 439.
Treatise on the Syphilis of Newborn Children and Infants at the Breast. By P. DIDAY.
2. *Histoire et Thérapeutique de la Syphilis des Nouveaux-Nés et des Enfants à la Mamelle.* Par — PUTEGNAT (de Luneville), Docteur en Médecine et en Chirurgie de la Faculté de Paris, &c.—Paris, 1854. pp. 216.
History and Therapeutics of the Syphilis of Newborn Children and Infants at the Breast. By — PUTEGNAT, M.D.
3. *Ueber die Syphilis der Neugeborenen.* ('Journal für Kinderkrankheiten?' BEHREND, Band xvii. s. 17; GUBLER, Band xix. s. 171; LUZSINSKY, Band xx. s. 274.)
On the Syphilis of Newborn Children. By MESSRS. BEHREND, GUBLER, and LUZSINSKY.
4. *De la Méningite Tuberculeuse, étudiée au point de Vue Clinique.* Par HENRI HAHN, Médecin de l'Hospice Joséphine à Aix-la-Chapelle, &c.—Paris, 1853. pp. 246.
On Tubercular Meningitis, examined in a Clinical Point of View. By H. HAHN, M.D.

5. *Traité Clinique et Pratique des Maladies des Enfants.* Par MM. RILLIET et BARTHEZ. Deuxième édition. Tome troisième. *Scrofule et Tubercules, &c.*—Paris, 1854. pp. 928.
- *Clinical and Practical Treatise on the Diseases of Children.* • By Messrs. RILLIET and BARTHEZ.
6. *Klinische Beobachtungen über die Gehirnkrankheiten der Kinder,— besonders über Gehirntuberculose.* Von Dr. HAUNER. ('Journal für Kinderkrankheiten,' Band xviii. s. 321)
- Clinical Observations on the Cerebral Diseases of Children, and especially of Cerebral Tuberculosis.* By Dr. HAUNER.
7. *Erlebnisse aus der Kinderpraxis.* Von Dr. JOSEPH BIERBAUM. Erste Abtheilung. *Krankheiten des Gehirnes und der Sinnesorgane: 1. Hydrocephalus Acutus.* ('Journal für Kinderkrankheiten,' Band xxiii. s. 167.)
- Results of Medical Practice among Children.* By Dr. J. BIERBAUM. Part First. *Diseases of the Brain and the Organs of Special Sense.*
8. *Lehrbuch der Krankheiten des Kindes in seinen Verschiedenen Altersstufen.* Von Dr. CARL HENNIG.* Erste Lieferung.—Leipzig, 1854. pp. 160
- Manual of the Diseases of the Child at Different Ages.* By Dr. C. HENNIG.
9. *Practical Treatise on the Diseases of Children and Infants at the Breast; including the Hygiene and Physical Education of Young Children.* Translated from the French of M. BOUCHUT, with Notes and Additions by FREDERICK HINCKES BIRD, F.R.C.S., &c.—London, 1855. pp. 776.

OUR first and earliest intercourse with the *arcana medica* of children, made through the mothers and nurses of the little patients of a public institution, went far in an endeavour to indoctrinate us into the popular belief that the whole and true nosology of the diseases of early life might be thus represented—*worms—teething*. When we refused this axiom, "water on the head" was not unwillingly added to the former; and when we again rebelled, the addition of *consumption* was thought equal to the surmounting of all emergencies. Had we been of this opinion, remained orthodox, and introduced no heresy and schism, we believe that with the above "quartevion" and the ordinary whooping-cough, chicken-pox, measles, &c. (the diagnosis of which is at once taken out of our hands), we might have gained as fair a reputation as did numerous others within the range of our out-patients' district for being "so famous for children." But neither our knowledge nor experience could permit it. We gave up lancing the gums and prescribing anthelmintics, and awoke to a full perception of the force of the true yet quaint saying of Goelis, of Vienna.* The next step saw us confessing to the truth that the diagnosis of pulmonary tuberculation in the child was occasionally one of the most difficult problems which came before us for solution. At this period, when we were so indignantly rejecting "worms" and "teething," "water

* Es ist ein verdammenswerther Schlendrianu grauer Unwissenheit, gewiss zwei Drittheilen aller Gebornen schon bei der Wiege das Schwanelied zu singen: das Kind hat Wasser im Kopfe.

on the head," and "consumption," as the chief sources of the sufferings of infancy and childhood, we were little aware how potent an ally the *four* might have brought into the field to attempt once again our definite subjugation. The support was not sought; motives of delicacy perhaps prevented the junction. That ally was *syphilis*. Time has passed on since we first commenced the warfare, and as we have become acquainted with the reputation of the weighty power of mischief alluded to, we feel glad to have escaped its influence on our minds until we had accumulated some experience of our own wherewith to balance it. Once (and but lately too) we confess to have been sorely puzzled as to whether or not, after all, our nosology was to be cut down to offshoots from the universal poison, syphilis. We read* with trepidation that the poison of syphilis "being once received into the blood, it remains there for years, and possibly—indeed *certainly for the rest of existence*"—that "the infected child must remain infected until death"—that "a considerable proportion of those diseases which pass under the name of scrofula are the produce of the syphilitic poison; are, in fact, *not scrofulous but syphilitic*;" and that lupus, kelis, lepra, and psoriasis, have their source in syphilis, which, in fact, is "a widely-spread, almost universal animal poison." We have recovered the shock, however, and although we dissent to the propriety of seeking the genesis of more than half "the ills that flesh is heir to," in the ineradicability of the venereal poison, we are not the less willing to admit the unfortunately too evident fact that it does "visit the sins of the fathers upon the children." Wide-spread and severe as such visitation is, however, we see the result of imagination rather than of truthful experience in such doctrines as the following would imply:

"Can," asks M. Diday, "hereditary syphilis manifest its action by any morbid effects other than the symptoms characteristic of the pox?"

"This question has already been discussed when alluding to pemphigus; it might equally come under consideration when treating of almost all the diseases of infancy, for there are few in the etiology of which syphilis has not been accused of taking a part more or less direct. Doublet attributed induration of the cellular tissue to it; Astruc, rachitism and mesenteric disorder; Bertier, certain hepatic affections; Pitschaft, obstinate wakefulness; Campbell, convulsions; Haase, hemi- and hydro-cephalus; Lamauve, acidity of the *primæ viæ*, apoplexy, and worms; and Levret, the dropsies of early age. In fine, I shall worthily conclude this list in recalling to mind that Sanchez perceived the results of the syphilitic virus in imperforation of the anus, hypospadias, and even in the green colour of the stools of the new-born child!!!" (p. 193.)

High in favour as syphilis stood in the opinions of the early writers as causative of very many of the diseases of children, for some years past it had lost ground in this respect, its aid being sought only to explain certain cutaneous manifestations, and some of the more obstinate evidences of scrofula occurring during the first infancy. But lately a revulsion has ensued, and we now find the dyscrasic poison under question not only appealed to as the source of a very numerous and diversified series of integumentary diseases, but of certain pathological changes in the tissues of the lungs, thymus gland, liver, and peritoneum, and as a poison which,

* On Syphilis, Constitutional and Hereditary, and on Syphilitic Eruptions. By Erasmus Wilson, F.R.S.

when once received into the current of the circulation, necessarily continues in the system until the end of life.

Since the time when Gaspard Torella (in 1498) affirmed that he had often seen the unwearied child infected by the sore breasts of its mother, and then bestow upon its nurses, who fondled and kissed it, the same unfortunate endowment, up to the late animated discussion at the Academy of Medicine of France, so wide a field of investigation has been gone over relative to congenital syphilis, as would demand a space for its analysis far beyond that we have here at command. We shall therefore confine ourselves within a narrow limit, laying the opinions of MM. Diday and Putegnat upon some prominent questions before our readers, and dwell a little in detail upon the late researches of MM. Dubois, Depaul, Cazeaux, and others, relative to the influence of the syphilitic poison in producing certain lesions of the abdominal and thoracic viscera.

That the whole subject is fraught with great interest is apparent, and as the forensic and "state police" relations of the question are becoming daily more urgent, the Medical Society of Bordeaux lately offered a prize for an essay upon the general matter. The treatise of M. Diday, now before us, was successful in obtaining it; whether that of M. Putegnat was one of the less fortunate we are not sure, but from some internal evidence we are inclined to think it may have been offered in competition. Both authors are known as men of ability, but in the present instance M. Diday has certainly outstripped M. Putegnat. The latter having taken for his motto the Horatian precept, *Quicquid praecepies esto brevis*, has so closely followed its spirit as (in our opinion) to have prevented himself from doing justice to the wide and, in relation to many points, much litigated questions before him. He is well acquainted with the literature of his subject, but is very far from having sufficiently unravelled its intricate web. On the other hand, M. Diday's statement, though by no means exhaustive, is yet as full, perhaps, as the form under which his treatise comes before the public would entitle us to expect. His book is a very fair *prize essay*. He has examined the evidence recorded in the literature of his own country, added to it the result of his own experience, and lays down his general laws accordingly. We need scarcely say, too, that the opinions and experience of the ex-chief-surgeon of the venereal hospital of Lyons should have some value in respect to our present subject.

M. Diday divides his work into five parts, under which are arranged the *etiology, symptomatology, prognosis, medico-legal relations, and therapeutics* of "infantile syphilis." He directs attention at the outset to the necessity of a definite nomenclature, and of drawing a rigid line of distinction between that *congenital* syphilis the child contracts during its intra-uterine existence from the formative or nutritive elements received from the parents, and that *acquired* taint it may receive at birth or afterwards, from absorption of the virus from any extraneous source. The two orders of phenomena thence arising are (strange as it may appear) constantly mixed up together, under the common term of "infantile syphilis." The laws of "hereditary transmission" having lately been discussed in these pages (vols. ix. and x.), when the works of Mr. Whitehead and Mr. Wilson passed under review, we shall at present merely

acquaint the reader with the doctrines expressed by MM. Diday and Putegnat.

Both agree to the proposition (M. Diday, p. 24, M. Putegnat, p. 106) that a father may bestow the syphilitic poison on the child without first of all infecting the mother, and, according to the former, (p. 26) though the immunity of the fetus is *possible*, when engendered by a father who having had syphilis is then, after a mercurial course, free from all open manifestations or symptoms, it would be an error to maintain its immunity to be certain. It is also M. Diday's opinion (p. 33) that if an attainted man has cohabited with a pregnant female, more particularly during the earlier periods of her gestation, we cannot affirm the immunity of the child, though the mother herself has not presented any signs of having been infected. The latter doctrine M. Putegnat (p. 107) is opposed to, believing the mother must be infected before the poison germ can be transmitted to the child. Relative to the influence of the *mother*, the first axiom of both writers is, that when infected before conception she exerts "a clear, patent, undeniable" (Diday), "almost certain" (Putegnat) deleterious action on the child. In stating their next and indisputable law—viz., that the mother becoming infected after conception attaints the child, they ask whether there is a period beyond which the risk of this attaint becomes lessened or negatived? M. Diday regards the cases he appeals to as supporting the doctrine that "syphilis contracted by the mother before the completion of the *fourth week*, or after the *seventh month*, has never been the cause of syphilis in the fetus" (p. 48). M. Putegnat thus writes:

"The fetus will be almost certainly attainted during the first three months of its existence.

"If the constitutional syphilis ensues only during the latter months, the fetus may escape, thanks to its own strong constitution, obtained from that of its father." (p. 112.)

Both writers, in admitting the law that father and mother alike being infected at the time of a fecund connexion, the chances are manifold that the offspring will be tainted too, yet deny the truth of the usually-received opinion that it must *necessarily* be so, or that the syphilization of the child is *obligatory*. Two later writers, MM. Maisonneuve and Montanier have even asserted that "the child has no chance of escaping the infection—it is fatal to it."

Passing to *acquired syphilis*;—it is believed by our authors that infection of the child by the mother during labour, or as the infant is passing *per vias naturales*, is, if not an impossible occurrence, yet a very rare and exceptional fact. Relative to the power of the lacteal secretion of an infected nurse in tainting the suckling, M. Diday enters at some length into the question, but arrives, we regret to say, at no more definite conclusion than the following:

"As regards myself and this litigated point, I neither absolutely admit nor reject anything. If, on the one hand, theory leads me to admit the reality of the influence, on the other, experience has not yet lent sufficient support to her suggestions. I await, therefore, the future, only appealing, in the first place, to the impartiality, and in the second, to the zeal, of after-investigators." (p. 78.)

M. Putegnat is more decided (p. 102), agreeing with Bertin, Bell, and

Ratier, &c., that there is no doubt about the power of the milk in conveying the poison germ of the syphilitic virus. The child may of course be said to be open to all the accidental causes of infection to which the adult is, still the state of infancy is found to be more liable to certain modes of infection than to others. The mechanism of the propagation is, under all circumstances, *contact*; but the conditions of the contact may be peculiar. M. Diday gives the following as common modes of infecting a child:—*a.* A nurse having a primary genital chancre touches her excoriated nipple, the latter becomes inoculated; the syphilized nipple is afterwards sucked by the child. *b.* Infected strangers incessantly, and with passion, fondling and embracing children, sleeping with them and pressing them next their bodies to keep them warm. *c.* Washing children with saliva, “the possible vehicle of a contagious principle,” with a sponge which has come in contact with a chancrous sore, or with water, previously used by an infected person.

A question of some import in its forensic as well as pathologic relations lately discussed and answered definitely in the negative by MM. Maisonneuve and Moutanier, is the following:—Can a fetus, deriving the syphilitic poison from the father, afterwards, or in a reflex manner as it were, infect its mother during its intra-uterine existence? For or against says M. Diday—

“Neither party being able to invoke facts absolutely conclusive, one is obliged to substitute quantity for quality, trusting that a happy chance may bring together before the observer the circumstances—so rarely united—from whose combination may spring, not a proof (that is here almost impossible), but a *probability*. . . . But would intemperateness at length invade this corner of science hitherto closed against its suggestions? I almost fear so, from the perusal of the most recently published work on venereal diseases, the treatise of MM. Maisonneuve and Moutanier. I shall not follow their example; I do not intend opposing to their negative and so absolute conclusions, others equivalently affirmative.” (p. 238).

M. Putegnat (p. 114) appears to be of one mind with M. Diday.

That the primary sores and chancres of a child may communicate their virus to the nurse, just in the same way as the poison passes from the organs of the female to those of the male during the frictions of coition, is a doctrine admitted by all syphilographers. But have the lesions of secondary syphilis the same communicability? or, to use the words of M. Diday—

“A child hereditarily affected with ‘mucous tubercles’ of the mouth, for example—can it bestow the same disease on a healthy woman who suckles it?” (p. 249.)

“Those who do not believe in the transmissibility of secondary accidents are obliged to explain *all* the facts bearing on our present question through the previous existence (forgotten or dissimulated) of a primary chancre in the infected individual. Their opponents are not forced to appear so exclusive . . . they admit the value of the doctrine to a certain extent, but deny that the latter can afford an explanation of *all* the instances in which syphilis is said to have been transmitted from the suckling to the nurse. And powerful reasons, it must be admitted, plead in favour of this eclectic opinion, which I myself adopt and feel necessitated to inculcate.” (p. 257.)

According to M. Putegnat, the transmissibility in question is *possible*, but that is all (p. 119).

We pass over the manifestations of syphilis in the child by the cutaneous and mucous surfaces, detaining the reader for a short time only in connexion with "pemphigus."

At birth, or a few days afterwards, some children are observed to have *bullæ*, chiefly on the palms of the hands and on the soles of the feet. In a ratio with the severity and extent of the eruption, the child gets weak and emaciates, cries continually, and refuses the breast. It appears to suffer much, and the general health is often so profoundly and quickly affected, that death results in a very few days. This disease, "infantile pemphigus," was formerly regarded as a syphilitic affection. But in 1794, Osiander protested against this view of the matter; the question excited no interest, however, until 1847, when M. Stoltz drew attention to the circumstance, that not only was "pemphigus" syphilitic in its nature, but he affirmed that it was the most common manifestation of the congenital vice that could be seen. The question was once more argued, and again forgotten until 1851, when it became the subject of a grand dispute between MM. Dubois, Ricord, Cazeaux, &c., in the French Academy. M. Dubois may be regarded as the representative of the affirmative, and M. Cazeaux of the negative phase of the argument. The main points rested on by the former are the traces of syphilis in the parents, and the frequent simultaneous existence in the child of other and characteristic manifestations of the venereal virus. M. Cazeaux, on the other hand, maintains that the description given of the so-called syphilitic pemphigus does not differ from that of the simple pemphigus of adults; that it appears at or soon after birth, when the known signs of hereditary syphilis do not become evident, but evince themselves at a later period; and that at the Ourcine, where a great many women are delivered syphilitised or fecundated by attainted men, no infant has yet, within his experience, been attacked by pemphigus. M. Diday remarks also:—

"It must nevertheless be admitted, that the number of cases of infantile pemphigus, in which the existence of venereal disease has been substantiated in the parents, is sufficiently great to make an impression on unprejudiced minds. Although the majority of the Academicians appeared fixed in doubt as to the nature of this eruption, did the mediate opinion of *conciliation*, we would ask— which regards it not as an immediate result of syphilis, but as an indirect one of the debility induced by the diathetic affection—obtain many suffrages? We ourselves adopt this solution; we shall endeavour to explain it, and give it precision." (p. 120.)

M. Putegnat appears to think the affection in question is frequently a direct result of the syphilitic poison, but admits the difficulty of distinguishing the specific from the simple form. Ample details will be found in the work of M. Diday, and in M. Bouchut's treatise the subject is also discussed, and in particular the differential diagnosis between the two forms of the disorder.

Those who have been curious in the history of syphilis, must have remarked that many of the older writers insisted on the liability of the liver and of the lungs to become involved in the syphilitic disease. From the time of Nic. Massa, in 1563, to that of the publication of the *Essays* of MM. Depaul and Dubois, in 1850-51, this fact, though frequently insisted on, yet made in the schools little impression. It is to these latter writers, and to M. Lagneau, that the credit is due for fixing the attention

of modern pathologists on this branch of the subject. Their inquiries, as well as those of M. Gubler, relate to the changes which the lungs, thymus gland, and liver undergo in consequence of the infection of the fœtus with the syphilitic poison. For the historical details of these questions we may refer the reader to the able papers by M. Gubler, in the 'Gazette des Hôpitaux,' 1848, and 'Gazette Médicale,' 1852, and which are given also in the 'Journal für Kinderkrankheiten,' as referred to at the head of this article, and from whence, if necessary, we shall quote. On reference to the work of Dr. Mauth,* previously reviewed in our fourteenth volume, it will be seen, that as far back as the middle of the seventeenth century, a Copenhagen physician, Simon Pauli, drew attention to the influence of the syphilitic virus on the *thymus gland*; and Lieutaud, 100 years later, renewed the observation. The matter was disregarded until 1850, when M. Dubois being struck with the death, a few days after birth, of children whose parents had had syphilis, and being unable to explain such result from the severity of the diseased manifestations of the cutaneous and mucous surfaces, determined to submit the viscera to a more attentive scrutiny than was usually bestowed upon them. Having done so, he recognised the existence of a particular disease of the thymus gland. Its characters may be gleaned from the following description of M. Diday:

"The thymus presents nothing extraordinary as regards its colour and volume; but on pressing after having incised it, little drops of a semi-fluid matter, of a yellowish-white colour, having all the appearances of pus, easily escape. M. Donné (to whom M. Dubois sent a little of the liquid for analysis) found, indeed, in it all the characters of true pus.

"In all the cases cited by M. P. Dubois, the purulent matter seemed disseminated in the tissue of the organ, and was not circumscribed or collected. M. Depaul has also met with this form in one instance; but in another, has remarked the thymus to have shown in each of its lobes a small cavity, filled with a grumous, yellowish, rather thick matter. In the latter case, the gland was likewise a little larger than usual; of five children affected with the disease, one was born dead, two lived but a few minutes, one survived six days, and one eight." (p. 147.)

In 1851, a memoir was presented to the French Academy by M. Depaul, giving rise to the well-known discussion we have before alluded to.† The author reminded the Academy, that as far back as 1837, he had, in the 'Proceedings of the Anatomical Society of Paris,' endeavoured to show, that the numerous small and scattered abscesses found in the lungs of new-born children, were but the signs of congenital syphilis. Since then he had met, not only with ample proof of the syphilitic nature of pemphigus, but with not less than *fifteen* cases of syphilitic abscess of the lung.

"I might say," said M. Depaul, "with almost certainty, that all, or at least most of, the cases of tubercles of new-born children, alluded to by Billard, Baron, and Husson, have been nothing but instances of congenital syphilis. What was taken for tubercle, were either single nodules of purulent infiltration, or true abscesses with more or less thickened walls."

M. Dubois, and those who side with M. Depaul, recall to mind how it has long been admitted, that a great number of children dying with syphilitic pustules, mucous tubercles, &c., have succumbed to pneumonia,

* Die Asthmatischen Krankheiten der Kinder. Erster Theil. Vom Verhältnisse der Thymus beim Asthma. Berlin, 1853.

† See also the previous number of this Journal, p. 33.

or have pulmonary lesions anterior to birth. They affirm that this pneumonia is not the simple lobular pneumonia of Barón, Billard, Husson, Cruveilhier, and Sestier, nor the "pulmonary collapse" of other writers, but a special affection, associated with other syphilitic phenomena, in children congenitally tainted. That the simultaneous presence of pulmonary abscess, thymic abscess, and frequently of pemphigus, in the same subject, is sufficient to prove the existence, at the least, of a cachectic condition of the whole frame. In 1852, M. Gubler carried this department of the pathology of congenital syphilis yet a step further, and placed before the profession an elaborate memoir on hypertrophy, or "plastic induration" of the liver, "not found as yet connected with any other diathetic malady than with syphilis." M. Gubler, and those who have followed him, describe the particular hepatic lesion as both of a general and partial character, and as evincing an alliance both to the secondary and tertiary periods, as the case may be. As we before remarked, these views (which we have but space thus cursorily to develop) of MM. Dubois, Depaul, and Gubler, have given rise to much discussion, though it has been chiefly, but not entirely, in connexion with the asserted syphilitic lesions of the lungs and thymus gland. M. Diday, alluding to M. Gubler, remarks—

"Happier than M. Depaul, he has found no difficulty in gaining admission for this new disease among the more incontestable effects of the syphilitic virus. The cases of it are so numerous, so satisfactory, that there has not occurred the slightest hesitation as to the position which should be accorded to the affection in the syphilographic catalogue." (p. 149.)

"The venereal character of the hepatic affection is, *primò facie*, rendered very probable from the constant coincidence of some of the preceding symptoms in all the children in whom it has been observed; but what completes the demonstration is, that M. Gubler has not met with it in union with any other diathetic disorder than with syphilis. Like him, MM. Trousseau, Horteloup, Gullerier, Depaul, Lenoir, and Lebert, have also met with analogous cases." (p. 152.)

On one point, M. Diday differs from M. Gubler:

"M. Gubler has attentively studied that particular alteration of the liver which co-exists with syphilis, but he has not assigned it a distinct place in the hierarchy of the successive accidents of this affection. In his eyes it is simply a lesion of the tertiary order, though at the same time it is one of the first symptoms which appears in new-born children, though, according to our author's own admission, no tertiary phenomenon (properly so called) is found simultaneously with it, and though it is the proto-ioduret of mercury, and not the iodide of potassium, which appears to be its specific! Are these considerations sufficient to admit of the very different interpretation which I have proposed of the same lesion? In my opinion, the induration of the liver is absolutely analogous in the fœtus to the induration of the chancre in the adult. It is the result of the transport of the virus which proceeds from the blood of the mother, and gives rise on its passage in the liver to that same organic reaction expressed by induration, that the virulent pus absorbed during coition determines around the chancre, and then in the first lymphatic gland it traverses. In this view of the matter, there could be no induration of the liver when the infection proceeds from the father. It is an hypothesis which will, of course, derive its future value from facts of this latter character. However, I shall at present abstain from giving a decided opinion as to its value. I shall only remark, that it meets with considerable support from the very great similarity that is established by appearance, feel, and microscopic examination between this condition of the liver and the induration of the primitive chancre." (p. 156.)

M. Gubler's paper terminates with the following remarks:

"From what has been stated, it will be seen that it is quite impossible to lay down the principles of a good and sure diagnosis. This much may be said, however, that when in a young syphilitically-tainted child we find considerable disturbance of the digestive functions, with marked general anæmia, enlarged volume, and altered consistency of the liver, we have reasons for thinking that 'plastic induration' of this organ is present. If with these symptoms those of *peritonitis* are associated, all doubt is removed."*

As a circumstance peculiarly worthy of record here, we may remark that, as far back as 1838, Dr. Simpson, of Edinburgh, drew attention in an able paper† to the occurrence of *peritonitis* in the *fœtus in utero*. Twenty-three cases (more or less detailed) of *peritonitis*, fatal either before or soon after birth, were recorded. In two of them (5, 9) the mothers had an attack of gonorrhœa during the period of utero-gestation, along with a syphilitic eruption in one instance (9), and ulcers in the other (5). A third (6) confessed that she had suffered from venereal disease; and the line of life pursued by others of the number (3, 4, 7) was such as appeared to expose them to syphilitic infection:

"Indeed," remarks Professor Simpson, "it appears to me highly probable, from the investigations which I have already made upon this point, that a great proportion of those children of syphilitic mothers that die in the latter months of pregnancy may be shown to have perished under the attacks of peritoneal inflammation."

The paper of Dr. Simpson's here alluded to is known to the Continental writers, but MM. Gubler and Cullerier affirm this syphilitic *peritonitis* to be, not a single primitive affection, but merely an accompaniment or sign of their "plastic induration" of the liver. According to the former—

"Nothing is rarer in early infancy than to meet with *peritonitis* divested of all association. It is a malady scarcely seen unattended by unipical phlebitis or with syphilis, and, according to my experience in the latter case, it is almost always connected with the hepatic alteration."

Now it is but fair to Dr. Simpson to point out that complications of this *peritonitis* are fully alluded to in his paper, but that, as regards the nature of the hepatic lesions, there is, no doubt, a difference between the views of the Scotch and the Continental writers. The former remarks:

"In a second case, the peritoneal inflammation was accompanied by hepatitis in the stage of softening, and incipient purulent infiltration; in another, the morbid changes in the same organ were of a more chronic character, the coats of the liver being opaque and somewhat thickened, and the organ itself reduced in size. In one instance there were found some of those small masses of inflammatory induration in the lungs which form the most common type of pneumonia in the *fœtus* and infant." (Op. cit.)

In the discussion at the Academy on the import of the changes in the lungs and thymus, M. Cazeaux, representing the dissentients to the new doctrine, first inquired whether the collections of pus in the lungs of newborn children might not be otherwise explained, than through the aid of syphilis, and then endeavoured to negative the particular support relied upon by M. Depaul in favour of his own theory:

* Journal für Kinderkrankheiten, Band xix. p. 194.

† Edinburgh Medical and Surgical Journal, Vol. xv. No. 137, p. 390: Contributions to Intra-Uterine Pathology, Part I.

"It is quite unnecessary," said M. Cazcaux, "for the support of my opposition, that I should prove to which group of diseases the pulmonary changes under discussion must belong, though not to syphilis. I say, it is not necessary I should prove this, but it is necessary that I should enforce this observation, that, without any overstrained and hypothetic explanation, perhaps a simple ordinary inflammation may be accepted as the true origin of these formations of matter."

Dubois, Ricord, Gibert, Danyau, Lagneau, and others, entered into the debate, but we must content ourselves with the following reply of M. Bouchut, as rendered by Mr. Bird, p. 730:

"M. Cazcaux appears to me to have far too much sacrificed the probabilities in favour of this exigency to the bare anatomical facts. He demands that a lesion should exhibit the nature of its first cause, whilst he himself knows very well that lesions, the result of constitutional syphilis, are not absolutely and always recognisable at first sight. If we reasoned in this way, we should return to the dark ages of Broussais, when every disease, even syphilis, was classed under the title of inflammation. We must not, then, tax anatomical facts too far, for fear of injuring them in the estimation of surgeons; and when they have no absolute signification, it is to the antecedent and concomitant circumstances to which we must look to determine their true nature. This M. Depaul has done; he has often noticed suppurating pulmonary nodules accompanying hereditary syphilis—he concluded that one might be the cause of the other, and he was right." (Op. cit.)

We must here bring to a close this department of our subject, strongly recommending the perusal of M. Diday's treatise to all inquirers into the interesting, important, though somewhat involved question of syphilis in the child.

We have so frequently in the pages of this journal* maintained the doctrine (first promulgated distinctly by MM. Rilliet and Barthez) of the rigid demarcation to be kept between that form of meningitis developed under the influence of the diathetic disorder, scrofula—granular meningitis—and that of a simple non-specific character, that it would be mere supererogation to enter now into the argument. In the concluding volume of MM. Rilliet and Barthez' great work, lately received by us, this doctrine is continued to be taught, and receives farther elucidation from the increased experience of the authors. To this volume we can refer with pleasure, as not only giving an admirable account of the meningeal disease just mentioned, but such an exposition of the general subject of tuberculosis in the child as is nowhere else to be met with. It appearing to M. Hahn that the attention of modern practitioners has been too exclusively directed to the morbid anatomy of infantile meningitis, that the therapeia of the disease has been regarded as if of secondary consequence, undertook "to place the treatment of tuberculous meningitis on a rational foundation, based on modern pathologic inquiry, and on the results of a lengthened experience." When about commencing his task, the Society of Medicine of Bordeaux proposed "a clinical inquiry" into the subject as a prize question. M. Hahn responded to the appeal, and the Essay now lying upon our table was the successful one in competition. The main endeavour of the author is to show that "It is not difficult to demonstrate that tuberculous meningitis is capable of a more or less perfect cure in a very great number of cases and at various periods of the malady." (p. 58.) Now this is a very bold and, we think, erroneous assertion, and

* See, in particular, vol. xiv. p. 482 et seq.

we should be disposed to reverse the statement, and say it is incapable of cure in by far the majority of instances and at all periods of the course of the disorder. This opinion coincides with that expressed by MM. Rilliet and Barthez in their last volume:

“Without going so far as Camper, who speaks of hydrocephalus as the *immediabile vitium*, we think that the scepticism of Frank has more to support it than has the exaggerated confidence of Golis, Heim, and Formey; and that the practitioner called upon to undertake the treatment of hydrocephalic patients will have far more defeats to lament than victories to celebrate.” (p. 508.)

We can yet well understand how loath many will be to accept so foreboding a prognosis as we feel called upon to give in reference to this malady, when men of high reputation and apparently ample means for observation, have maintained the contrary. On both sides may be ranged names of high repute. M. Hahn remarks:

“The prejudice which attributes a character of incurability to tuberculous meningitis serves only to shackle the progress of medical art. But we have sufficiently elucidated this question in our fourth chapter, and have there demonstrated that the disease is, in a very great many cases, susceptible of cure.” (p. 110.)

Now, where is the cause of such discrepancy of opinion to be sought? Is it in the greater therapeutic ability of the practitioner, and a more successful method of treatment, or in a difference of diagnosis? We believe in the latter. Cerebral congestion and erethism, simple meningitis, &c., have been mistaken for the more formidable disease, and recoveries from them recorded as if from the latter. MM. Rilliet and Barthez thus express themselves:

“The generally admitted danger of the malady obliges us to regard as apocryphal the great number of cases of cure published by authors. In truth, a rigid analysis proves that a considerable amount of them relates to diseases very different to meningitis, and having only a very coarse symptomatic analogy with it.” (p. 510.)

Dr. Bierbaum reduces “acute hydrocephalus” to three forms—viz., simple meningitis, granular meningitis, and “a cerebral affection offering the same symptoms during life, but after death neither the lesions of simple nor of tuberculous meningitis. This affection appears to have, as its basis, a dynamic disturbance of the cerebral activity, induced by irritation.” (p. 169.) Thirty cases are detailed, and the *prognosis* afterwards thus spoken of:

“We have already seen that this cerebral disease had a fatal issue seventeen times in our thirty cases. There died, consequently, more than one-half of the affected children. In other instances I was more successful, and saved a half. But even from this rate of mortality it must be concluded that this cerebral affection yet belongs to the most lethal of infantile maladies.” (p. 225.)

The author admits that the granular variety of his “acute hydrocephalus” is by far the more fatal one, yet he afterward (p. 248) asserts that the differentiation of the general disease into a granular or tuberculous and a simple meningitis, has been of no especial advantage to the treatment, as the same indications are present in both varieties. Now, we believe there is a general opinion prevalent among modern pathologists, cognizant of the difference between the two, that the simple form is by far the more amenable to treatment, and notwithstanding the not very

cheering opinion given by MM. Rilliet and Barthez, we continue to think so.

"If," say these writers, "the absence of the tubercular element, and the outbreak of the disease in the midst of perfect health, offer legitimate hopes of seeing the meningitis terminate favourably, it must not be disguised, on the other hand, that the extent of the inflammation, and the rapidity of its progress, leave but few resources to the practitioner, and even less to time." (vol. i. p. 126.)

We will now revert to M. Hahn, who comes to offer us a more favourable opinion as to the curability of the granular variety. The author allows no *invariable* form of therapeia to be possible, but, making three divisions of the disease according to its form and progress, lays down a treatment most conformable to each. It may be thus indicated:

A. Antiphlogistic regimen and diet, depletion by bleeding or leeches, cold applications to the shaved head, sinapisms and pediluvia, calomel, blisters to distant parts of the body, blister to scalp or nucha, and use of revulsives.

B. Occasional leeches, cold lotions to head, calomel, iodide of potassium, digitalis, revulsives, &c., as before. •

C. No treatment of avail.

One of the most particular points in M. Hahn's treatment is certainly the prominent use of powerful revulsives, and his advice to cause a suppurating sore on the vertex or at the *nucha*, is peculiarly his own, so far as the stress upon it is concerned, although M. Bessières some time ago drew attention to the importance of the sore from a blister, if suppurative.

"We are convinced," writes M. Hahn, "that we possess in the antimonial ointment a therapeutic measure still more efficacious; and that, in the latter stages of the malady, its employment offers out far more chances of success than the use of blisters." (p. 125.)

Severe pustulation of the scalp is induced, sometimes, as we have ourselves seen, even gangrene of it, and M. Hahn states that "it may happen that the bones of the skull shall exfoliate in one or two places. But this is of extremely rare occurrence, and we are aware of only one example." (p. 131). Without acceding to M. Hahn's general proposition, we willingly do so to the particular one concerning the value of suppurative derivation. We have used it freely—twice even to the extent of causing a portion of the scalp to slough—and have no hesitation in saying that we consider the therapeutic measure of counter-irritation to the nucha and vertex as having the most—little as it may be—hold over the progress of granular meningitis. In answer to the asserted cruelty of the treatment, M. Hahn makes answer "Aux grands maux les grand remèdes." (p. 133.)

MM. Rilliet and Barthez thus refer to the practitioner of Aix-la-Chapelle:

"Dr. Hahn has published some interesting observations relative to the cure of very severe cerebral diseases, from the result of frictions with antimonial ointment on the previously shaved head. Relative to the greater portion of the facts reported by the author, the diagnosis is not sheltered from all contestation." (vol. iii. p. 524.)

As supporting his views on *curability*, M. Hahn refers to the recent observations of Romberg, in the 'Deutsche Klinik,' and concludes his

essay by some remarks on "white softening," a point more fully considered in some papers in the 'Lancet,*' by the writer of the present article.

Dr. Hennig's work may be found by the German student a useful manual. It is quite a compilation, written in a dry, compressed style, but seeks to take a complete view of the present state of infantile pathology.

The English reader is much indebted to Mr. Bird for bringing M. Bouchut before their notice in so complete and satisfactory a form. We have before passed M. Bouchut in review, and as the chief additions to the present work consist in quotations, in the form of notes, from the various articles on pædiatrics which have appeared since 1819 in this Journal, and from the 'Lectures' of Dr. West, we have nothing left us to perform but to recommend very strongly Mr. Bird's edition of M. Bouchut, to the student of medicine in particular.

W. Hughes Willshire.

REVIEW V.

Report of a Committee assembled by General Orders, Commander-in-Chief, dated Sept. 16th, 1845, under Instructions from the Right Hon. the Governor-General of India in Council.—Agra, 1847.

AT first sight it may appear rather *moutarde après diner*, to draw attention to a Report made, no less than eight years ago, for the purpose of determining the propriety of constructing a canal in the north-western parts of our Indian empire. The Report, however, contains facts of importance bearing upon the whole question of endemic influences, which we think fully justify our introducing a summary of its contents to our readers, though at so late a period. The heading in no way indicates the duties of the committee; these are given as follows in the General Orders:

"The Committee are to assemble at such places and on such dates as may be fixed by the President, for the purpose of reporting on the causes of the unhealthiness which has existed at Furruckabad and other portions of the country along the bank of the Delhi Canal; the Committee will also report whether an injurious effect on the health of the people of the Doab is or is not likely to be produced by the contemplated Ganges Canal."

The question of the healthiness or unhealthiness of the localities alluded to, is not one of sufficient general interest to justify its being brought before our readers, were it not that the medical member of the committee, Mr. T. E. Dempster, has suggested a test for the presence and intensity of malarious miasm, which, in the inquiry before us, seems to yield positive and satisfactory conclusions, and the value of which appears to have been fully established in the district in which the investigation was instituted. The object was to ascertain whether the Great Ganges Canal, which now unites the Ganges and the Jumna, but which, at the time of the inquiry, was in contemplation, would tend to increase disease, and thus prove not a blessing but a curse to the inhabitants of the country through which it was to pass. Opinion was at the time much divided on the subject; one party, chiefly composed of medical men, held that all the unhealthiness of certain districts was solely to be ascribed to the influence of existing canals, and canal irrigation; another as confi-

* For October, 1853.

dently maintained that no sanitary question at all was involved in the construction of works of this nature.

In a country where there are no means of obtaining any medical or vital statistics, and where any sanitary research is regarded by the inhabitants with such suspicion that they use all their efforts to conceal facts or mislead the inquirer, nothing but a test that was beyond the control of the natives, could serve to elucidate the presence or absence of the marsh poison and its effects upon the system. Mr. Dempster proposed to examine the spleen in a certain number of individuals, and by the presence or absence of an enlargement to determine whether or not the residents in a certain locality had previously suffered from the endemic fever. The results of a laborious and extensive inquiry have most satisfactorily proved that the test is a reliable one. Major Baker and Mr. Dempster—

“Examined the irrigated and unirrigated districts on both banks of the Jumna, and followed the proposed course of the Ganges Canal for eighty-three miles—viz., from Hurdwar to the latitude of Meerut. In the course of this inquiry they travelled about 1400 miles. They visited more than 300 inhabited localities, and personally examined upwards of 12,000 individuals of all ages.” (p. 1.)

The mode in which the examination was conducted consisted in selecting, at each place visited, twenty children and twenty male adults, taken at random from the population. Those avowedly diseased were not encouraged to come forward, and only admitted into the lists when there were no others. Subjects from all castes were taken; and while Mr. Dempster conducted the medical examination, Major Baker entered the results in his note-book. Mr. Dempster observes, that—

“As the great object was to make use of an unequivocal but easily-applied test, no case was ever registered as ‘spleen,’ unless I had so distinctly felt the enlarged organ, that it could not be confounded with any other disease. When the abdomen was natural, and the muscles soft and yielding, a satisfactory examination was generally obtained in the erect position; but if the belly was rigid, and the region of the spleen tumid, but not clearly defined, the subject was put flat on his back, with the knees bent and raised. If an enlarged spleen was not discovered after a moderately careful examination so conducted, the person was registered free from the disease. Sometimes, though rarely, really doubtful cases were met with, which could not at once be pronounced upon; these were put aside, and others examined in their stead.”

Five different degrees of size⁶ were noted, according as the spleen projected more or less below the hypochondria. Numerous tables, embracing a minute analysis of the results obtained by this elaborate process, are appended, while a coloured map of the localities traversed shows at a glance the per-centage of enlarged spleens occurring at each of the places visited. We are unable to reproduce all the tables as we would wish to do, although extremely valuable in showing the accuracy of the conclusions arrived at, and the care with which the data have been collected. We have examined them all, and cannot but express our opinion, that “the chain of induction” formed by the writers of the Report is complete, as regards the locality and the morbid influences in question. The following table contains a summary of the whole inquiry:

	Percentage of enlarged spleens. Adults and children of all classes.	Percentage of adults suffering from fever.		Average depth of water from surface of ground, in feet.
		1844.	1845.	
Irrigated from the Western Jumna canals.	{ Within half a mile of the canal	58	45	11
	{ Distant more than half a mile	49	49	18
	{ Within half a mile of the canal	44	38	27
	{ Distant more than half a mile	29	34	48
Irrigated from the Eastern Jumna canals.	{ Within half a mile of the canal	16	36	102
	{ Distant more than half a mile	39	41	92
	{ Within half a mile of the canal	18	31	118
	{ Distant more than half a mile	20	39	8
Irrigated from wells in the high land of the Dooab.	{ Within half a mile of the canal	22	47	13
	{ Distant more than half a mile	59	54	8
	{ Within half a mile of the canal	47	53	14
	{ Distant more than half a mile	25	40	24
Unirrigated.	{ Within half a mile of the canal	18	30	31
	{ Distant more than half a mile	8	31	24
	{ Connected with the canal	44	52	0
	{ Unconnected with the canal	29	61	0
Naturally malarious localities	{ Unconnected with the canal	11	28	88
	{ High or bunger land*	3	30	46
	{ Gauges khadir†	21	42	25
	{ Near head of Eastern Jumna canal	6	48	0
Naturally malarious localities	{ Nujunghur, Jhucst	44	59	16
	{ Valleys of Jumna and Hindun	34	42	14

* *Bunger*: The high and firm bank of the river bounding the *khadir*.
 † *Khadir*: The belt of moist, low, and often fertile land found alternately on one or other side of large rivers in these provinces. During the rainy season much of this land is submerged, and in it the river frequently alters its channel.
 ‡ *Jhucst*: Shallow pools of water, often very extensive during the rainy season.

It will be observed that in each subdivision of the table the smallest number of spleens and the correspondingly small number of previous fever cases, occur in the inverse ratio of the proximity to the river or the canal. The ratio of the previous fever cases does not quite agree with that of the number of enlarged spleens, but possibly if one could determine the intensity of the attacks, we should find that a more definite ratio could be established between the attacks and the spleens.

It is scarcely possible, on looking over this table, and knowing the care used in making the inquiries upon which it is based, to refuse to admit that there is a direct relation between malaria, the amount of splenic enlargement, and canal irrigation, in the north-western provinces of the Bengal Presidency. The facts thus elicited are of general interest, and may serve to suggest in other parts of India, other colonies, and in European countries, the utility of ascertaining whether the human body offers similar indications with regard to the influence exerted by definite poisons. There are other glandular organs which might be employed as tests as well as the spleen: thus, Professor Alison, in a former number of this Review,* observes:

“It would seem to be nearly ascertained, by the observations chiefly of a clerical philanthropist, M. Billier, Archbishop of Chambery, that the origin of the poison producing goitre and cretinism is nearly analogous to that of malaria; and no doubt the special conditions necessary to its very partial development will one day be made out.”

We do not find that Mr. Dempster rides his hobby too hard. On the contrary, the great cautiousness with which he expresses himself with regard to the spleen test, strengthens his case considerably. He distinctly says that he does not assert it to indicate the presence of the remote causes of *all fevers*, or even of all pure endemic diseases of this class. While he admits that there may be different kinds of malaria, giving rise to fevers of different types, and having different complications and consequences, or that common continued and typhoid fevers become mixed up with and modified by fevers of local origin, he is fully persuaded that *marsh malaria* in its extended sense may be measured by the spleen-test, and that canals and canal irrigation have a proper connexion with that poison alone.

Having shown that the country was comparatively free from the marsh malaria previous to the formation of the Ganges Canal, it would be of extreme interest to know in what proportion it had manifested itself since the canal has been dug. It would not necessarily present exactly the same degree of intensity which it was found to manifest in the region of the Delhi Canal, because much depends upon the character of the soil and the extent to which at the same time drainage is introduced. On this point the committee remarked as follows:

“In the course of our inquiries on the existing canals, we have found salubrity to depend, in a great measure, on the nature of the soil and the efficiency of the surface drainage. . . . If attention to drainage be made an absolute condition in the benefits of the canal, an improvement, rather than a deterioration, of the general salubrity may in many instances follow the introduction of canal irrigation.”ⁿ
(p. 5.)

* April, 1854, p. 337.

The Committee considered it evident that the Doob, through which the Ganges Canal was to pass, offered facilities for drainage not possessed by the region of the Delhi Canal.

A very remarkable instance proves the general accuracy of the spleen test, and the peculiar relation it bears to the marsh poison: the following observation, while it shows that poison to be unconnected with the poisons that result from the presence of effluvia generated where large bodies of human beings are congregated together, but unexposed to marsh effluvia, also confirms the fact that the marsh miasm is the real cause of the splenic enlargement:

“The city of Delhi appears, at first sight, an instance in which the test failed; but on careful examination I think it will only be found to furnish a striking confirmation of its general accuracy. The medical topography of the city, civil station, and military cantonment of Delhi, is an extremely complicated subject, and involves a great variety of important considerations; but I need only briefly notice in this place the following particulars:

“Within the walls, and especially in the most dense and crowded quarters of the city, there were comparatively few indications of pure malarious disease. This accords with what has often been remarked in other countries—viz., that the high walls and narrow, crowded, smoky streets of large cities are frequently a safeguard against marsh miasma, although other causes of disease may abound in such situations.

“In the cantonment bazaar and suburbs outside the walls, a considerable amount of spleen disease was found. But when we proceeded to examine the villages situated on the verge of the low moist ‘khadir’ land, immediately in front of the sapper lines (a position now abandoned, in consequence of its extreme insalubrity), the test at once pointed out malaria in its highest intensity.”*

We cannot conclude this notice of the valuable inquiry carried out by Mr. Dempster and Major (now Colonel) Baker, without expressing a hope that we may again hear from them, and that the above summary of the results arrived at by those gentlemen may excite others to institute similar investigations, even though they may not have the means or the opportunity of operating upon so large an area, or examining an equal number of human beings.

REVIEW VI.

1. *On Stricture of the Urethra, and Fistula in Perineo.* By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, &c. &c. Second edition.—*Edinburgh, 1855.* pp. 109.
 2. *Traité Pratique des Rétrécissements du Canal de l'Urètre.* Par M. le Dr. J. F. REYBARD. Ouvrage couronné par l'Académie Impériale de Médecine qui lui a décerné en 1852 le grand prix d'Argenteuil.—*Paris, 1853.* pp. 600.
- A Practical Treatise on Strictures of the Urethral Canal.* By Dr. J. F. REYBARD. The work selected for distinction by the Imperial Academy of Medicine, which decreed to him in 1852 the great Argenteuil prize.

It was the recorded opinion of an old and experienced hospital surgeon who flourished at the commencement of the present century, that organic

* Appendix B, by Mr. Dempster, p. vi.

stricture of the urethra is a disease of considerable rarity. Without giving an unqualified assent to this proposition, expressed as it is in terms which are at least sufficiently vague, it may perhaps be correctly affirmed that the vulgar belief in the commonness of the affection is an extremely erroneous one. This is not a remark made without consideration, but a conclusion based upon a tolerably large experience of the misapprehension which exists in the minds, not only of patients, but sometimes even of members of our own profession, respecting its existence in particular cases. How frequently it happens that we meet with individuals who, having for some time experienced symptoms indicating more or less of undue irritability about the urethra or bladder, labour under the impression that they are the subjects of stricture, and have, perchance, been treated pretty extensively for it too, especially if they have fallen into the hands of extra-professional pretenders, while they are, nevertheless, perfectly free from any narrowing of the urethral canal, either of a permanent or a temporary character. On the other hand, it is manifest that a disease, some veritable examples of which may always be found in the wards of any one of our large metropolitan hospitals, cannot be esteemed a very uncommon one.

A glance at the numerous works which have been written during the last few years, both in this country and in France, respecting urethral stricture and its consequences, might certainly tend to confirm the popular impression. A prolific literature in connexion with any particular subject in pathology is not, however, to be accepted as absolute proof that the matter discussed either possesses very ample limits, or is presented for investigation in a very extended field. It quite as commonly indicates that considerable difference of opinion exists in relation to some question arising out of the subject; and thus we may observe, that as difficulties and discrepancies become in course of time cleared up, the still remaining points of difference alone afford themes for authorship and debate, until they in their turn are settled to the satisfaction, at all events, of the existing generation. Yet most certain it is that, when these discussions are lost sight of and forgotten, a future age will again witness revivals of the old contest, and many a dogma bearing the hitherto unquestioned seal of ancient authority will be again examined, disputed, and perhaps overthrown, by later and better-informed investigators.

At the present moment, notwithstanding all that has been done and written concerning the subject in question, we can point to no fact more strikingly illustrative of these remarks than the nearly cotemporaneous appearance of two works, the titles of which are placed at the head of this article—the productions of two labourers in their respective fields—no theorists merely—both undoubtedly seeking truth with zeal and earnestness, yet arriving apparently at the most opposite results—each in the strongest confidence that his own achievement will alone bear the test of time and experience. And yet with great seeming—nay, real difference—it is pleasant to observe—or to think, at least, that we can trace—the one unchangeable form of truth in both, overshadowed though it may be by much of error, as we believe, in one of the two productions before us. We make no apology for thus placing, but for a moment only, in one category, as it were, our illustrious *confrère* of the north, to whom modern

surgery is so greatly indebted, and the French author whom the acquisition of a literary prize has for the first time rendered famous. Both assume to have made a large advance in relation to the pathology and treatment of the subject of which they treat; and the claim of the latter, although not enhanced by any weight hitherto attaching to his own name, is endorsed by the favourable verdict of no less a body than the Imperial Academy of Paris.

We shall attempt to give a brief sketch of each of the works before us, taking first the volume with which, it may be presumed, we are in this country least acquainted.

It may be known to most of our readers, that the late Marquis d'Argenteuil devised property in France, affording annual proceeds of nearly 2000*f.*, for the purpose of founding a prize to be devoted to the object of encouraging and rewarding improvements in the mode of treating stricture of the urethra, and that he gave authority to the Imperial Academy of Medicine at Paris to adjudicate upon the claims of all who might become candidates for this distinction. This body decided on admitting competitors to the concours once in six years, so as to afford a fair interval for permitting the annual income to form, by accumulation, an attractive prize. The first concours was held in 1846, when M. Leroy d'Etiolles was the successful competitor; the second took place in 1852. On this latter occasion, with which we are now concerned, the commission of the Academy appointed to decide on the merits of the respective candidates, was composed of MM. Ricord, Robert, Roux, Gerdy, Bouvier, Huguier, Langier, Larrey, and Grisolles. The prize amounted to 12,000*f.*, and was awarded to M. Reybard, formerly of Lyons. This gentleman's essay, enlarged from its original dimensions, forms the work in question, and contains an exposition of his views and experience of stricture in general, but relates, for the most part, to his own peculiar mode of treatment, extending altogether to about 600 pages.

The foundation of M. Reybard's method is to be traced, at the outset of the work, in the views which he takes of the causes and constitution of organic stricture; and these must be briefly explained, in order to appreciate the *rationale* of his mode of treatment. Moreover, he himself lays considerable stress upon the necessity for carefully following his exposition of this part of the subject.

Passing over the first book, in two chapters, devoted to the consideration of the anatomy and physiology of the urethra, and containing nothing requiring remark, we find the second book opening with the usually accepted definition of stricture, and an enumeration of the causes of the affection. The first class of these comprehends traumatic lesions of all kinds, whether occasioned by accidental sections, lacerations, and bruises of the urethra, or by surgical operations with instruments and chemical agents. This is divided into three groups—Ulcerations, Incised Wounds, and Contusions. Under the first head, in order to show the relation between ulcerations of the urethra and the subsequent formation of stricture, the author details certain experiments which he made upon the urethra in dogs. He passed small quantities of nitrate of silver to a certain spot and left it there, noting the results during life, and killing the animals six weeks afterwards to observe the pathological changes which had

been induced. As might be expected, the loss of substance had resulted in the formation of a cicatrix, and the production of considerable narrowing of the canal. On three other dogs he performed the same experiment, subsequently employing dilatation, with little or no benefit, during a considerable period. In discussing the second group, that of incised wounds, M. Reybard is at some pains to show, that while transverse sections of the urethra are commonly followed by considerable narrowing, longitudinal incisions of the canal have no such effect. In his remarks upon the third group, that of contusions, it is not necessary to follow him.

The second class of causes to which M. Reybard invites attention, is that which includes urethral inflammations of all kinds. This morbid action, he says, all authors have agreed to consider as the chief and most common cause of organic stricture; but they have not, he affirms, inquired into, or at least have not discovered, either the relation which subsists between that action and the production of the abnormal tissue constituting stricture, nor the physical characters and properties which it possesses. He claims solely for himself the merit of having supplied this desideratum in our knowledge, and devotes about sixty pages to an elucidation of his views of this subject.

The sum of these may thus be very briefly stated, and, for the most part, in the author's own words:

"Inflammation is a cause of stricture upon one condition only—viz., that of giving rise to an abnormal tissue of new formation, and this with such an invariable certainty, that the same cause in the same circumstances will always produce an identical effect." (p. 94.)

The inflammatory process, in order to give rise to stricture, must extend beyond the mucous membrane; it involves, also, the sub-mucous tissue, and, it may be, the inner sheath of the corpus spongiosum, or even the entire structure of that body. Stricture is, therefore, the more likely to succeed a urethritis "in proportion as the inflammation is profound, realized, and chronic." (p. 97.) Having declared it to be impossible that mere congestion of the part can form the condition which we understand as stricture, and that the latter has no existence except in the presence of an organized deposit in or around the urethral walls, and resulting from inflammation, he next examines its anatomical and physiological characters:

"The tissue which enters into the formation of stricture is," according to Reybard, "that of granulations (*tissue inodulaire*) as met with in ordinary cicatrices. . . . It forms itself at the expense of those plastic products which inflammation has evoked and deposited in the substance of the urethral walls." (p. 112.)

The thickness of the deposit is generally very inconsiderable, less than it is customary to suppose (p. 116); its consistence is more considerable than that of the natural structures, and it augments with time. It exhibits certain physiological properties, which possess much interest and importance in the estimation of the surgeon—viz., "retractility," "extensibility," and "elastic retractility." Thus it is asserted by Reybard, that all strictures have a natural, inherent, and unconquerable disposition to contract in the course of time, or become confirmed by age, whether sub-

jected to dilatation or otherwise, and this he designates "retractility;" which contraction is described as "an atrophic process," common to it and to ordinary cicatricial tissue. By "extensibility," he intends that property by virtue of which all, excepting contractions of the most confirmed character, may have their calibre temporarily increased by dilatation; and by "elastic retractility," that tendency to return, more or less speedily—which all strictures exhibit after treatment by sounds or bougies—to their original abnormally-diminished calibre. All cases, therefore, exhibiting these properties are termed by Reybard "dilatable strictures," while those confirmed examples which do not admit of extension are called "non-dilatable." In using the term "dilatable," he desires it to be clearly understood that he does not intend to describe by it a stricture *curable by dilatation*, believing that none are, or can be, so cured—that is, placed beyond liability to return; a doctrine deduced from two propositions involved in the foregoing—the first, that all organic strictures are invariably constituted by the tissue above described; the second, that this tissue invariably possesses an inherent tendency to contract or shorten itself.

This deduction leads him, therefore, to propose as the only cure for strictures, whether slight or considerable, old or recent, a free incision of all the tissues composing the walls of the urethral canal, so as to divide completely the strictured part, and a portion of the healthy structures, both before and behind it.

Before, however, entering further into the details and *rationale* of the operation proposed, it is only fair to English pathology to remark, that the intimate nature and physiological properties of stricture, as expounded by M. Reybard, possess no novelty for surgeons on this side the Channel. The deposition of plastic matter in and around the urethral mucous membrane, and not a condition of "vascular engorgement," which, according to Reybard, had hitherto been considered (in France?) the proximate cause of stricture, has long been recognised in this country as the uniform and essential condition which produces permanent contraction of the canal. This fact was demonstrated by Sir Everard Home in an elaborate paper published in the 'Philosophical Transactions' in the year 1820. The same fact has been subsequently insisted on and illustrated by Sir B. Brodie and others. We may perhaps be also permitted to add, that in our own work, presented to the Royal College of Surgeons in 1851, the histological elements of stricture were carefully examined and described, and it was shown that they were identical with those of the cicatrices of burns, and of the fibroid tissue which results from inflammatory action occurring in internal organs; an additional proof of the identity of the material in the two cases, not adopted by M. Reybard. Indeed, this author is obviously quite independent of any aid which microscopical investigation might afford him in the study of the minute anatomy of the stricture elements, as may be learned from his account of them in a case examined by himself, and cited in elucidation of this very subject. Thus he writes: "The stricture was fibro-cartilaginous; and its tissue—of a lustrous white—had the consistence and, without doubt, *the same organization* as that of cartilage!" (p. 108.) But notwithstanding that we have long entertained here these views of the constitution of stricture, we have

not been led to accept as a necessary consequence the therapeutical conclusion at which he arrives. Admitting that all organic strictures are constituted by this abnormal tissue, and that its obnoxious property is to exhibit a natural tendency to increase rather than to disappear, it by no means follows that a complete section of the tissue should be the only mode of cure remaining in our hands. That the judicious use of dilatation is a perfect cure for some cases of organic stricture, the most experienced surgeons of this time will attest. They do not explain the fact by attributing to the bougie or catheter an action which is mechanical merely, but are impelled to the belief by facts which experience has made patent, that some action, so-called "vital," must arise from the pressure exerted upon the abnormal elements of the stricture; an action which may be presumed to dissipate them, as it has been supposed, by absorption, and, consequently, to destroy the contractile tendency which before existed in or about the affected portion of the urethra. This influence of the sound M. Reybard does not recognise; he believes that dilatation is never anything more than a mechanical agent, and therefore temporary only in its action; and that its employment rather tends to augment the stricture, by inducing fresh chronic inflammation, and consequent renewed deposit of the obnoxious material around. Hence he proposes an invariable resort to the knife for the cure of stricture. But researches in the morbid anatomy of stricture, a branch of inquiry pursued much more extensively in this country than in France, as our respective museums testify, bid us demur to the dogma that the organic obstruction is invariably constituted by a layer of the plastic matter described. A stricture which is recent and slight, but which, nevertheless, M. Reybard would treat by incision, and by a long train of subsequent manipulations, in order to keep that incision open, as we shall hereafter see, is not necessarily the result of plastic matter *surrounding the canal*. A much less considerable deposit than this—indeed, the mere thickening of the mucous membrane of the urethra, or a deposit affecting only one of its sides, or a little cord of fibroid matter stretching beneath the membrane for a short distance, are, either of them, sufficient to cause a very notable and inconvenient contraction of the canal. These conditions we have verified by autopsy, and have elsewhere described as constituting the most simple, perhaps incipient forms of stricture, each probably constituting an example of the disease completely curable by dilatation: in other words, there exists good ground for believing that many of those cases which we are satisfied do completely disappear under dilatation, and do not return, are cases possessing the anatomical characters described.

Of late years, however, surgeons in this country have been more and more inclined to admit that dilatation fails to effect a complete cure in a considerable number of cases, and have therefore resorted to various contrivances for extirpating an obstinate obstruction by some method of cutting or cauterizing; but we think few, if any, will be inclined to coincide with M. Reybard's absolute rejection of the bougie as a therapeutical agent, and will naturally feel somewhat startled at his proposition, and his practice of reserving it solely for the purpose of making room for the knife; or for "accustoming," as he has it, the urethra to the presence of foreign bodies, previous to his operation of urethrotomy. On the other

hand, most practical men will agree with M. Reybard's, repudiation of certain methods which, as they were originated in his own country, have always found the greatest acceptance there—viz., those of attempting to estimate the nature of a stricture by means of "model bougies," particularly the "bougies à empreintes" of Ducamp. It is not without satisfaction that many will observe the sign of a return to a simpler and sounder practice on the part of our brethren beyond Channel, in the following notice of these instruments: "Nothing," says M. Reybard, "can be more deceptive than this method of appreciation, and we shall see further that it has led to the most deplorable abuse of caustic" (p. 144): an agent the employment of which, let it be remarked, he denounces on several occasions in very forcible language. But he regards as a useful means of estimating the extent and situation of a stricture, the small sound with a bulbous extremity, made in various sizes, such as has long been employed here, of a form somewhat modified from the original pattern of Sir Charles Bell.

In entering upon the much discussed subject of treatment, M. Reybard announces his method in the following somewhat positive terms:—"My principal aim is to demonstrate that urethrotomy practised from within outwards, and, according to the method which I have proposed, constitutes the *only curative treatment* of stricture." (p. 205.)

After premising that all intra-urethral incisions, such as those made by Stafford and others in this country, by Amussat, Leroy, Ricord, Tanchou, Delcroix, and others in France, whether described as scarification or urethrotomy, have been radically defective, at the best palliating, but never effectually removing the complaint, he repudiates their employment except when necessary to ensure space in the urethra for the passage of his own cutting instrument, which, as will be hereafter seen, is almost large enough to fill a healthy urethra of average capacity.

The practice of M. Reybard's method is conducted as follows. Having passed a sound through the stricture, and dilated it, until it will admit with tolerable ease an instrument of the size of No. 9 or 10 of our scale, he passes his urethrotome, which consists of a canula, containing a rod having attached to its distal end a slender blade nearly or about an inch in length, so arranged that this latter can be exposed with ease and certainty to any required extent. This blade having been carried, while concealed in the canula, completely through the stricture, is then exposed and drawn towards the operator, so as to divide the whole of the stricture, together with the entire thickness of the urethral walls, for an inch behind and an inch before it, making usually a wound about three inches in length. He advises that the blade should then be thrust backwards and forwards three or four times, so as to ensure the effectual division of the tissues, both in respect of length and depth. The hæmorrhage, which is frequently considerable, is to be arrested if necessary by passing down to the incised portion of the urethra a hollow bag of caoutchouc rolled into the form of a bougie, and capable of being inflated with air or water while *in situ*. The subsequent treatment consists in passing a full-sized bougie, or some special dilating instrument, twice a-day on not less than thirty or forty consecutive days, in order to maintain apart the borders of the wound in the urethra, to prevent union by the first intention,

ensure the production of granulations, which shall afterwards constitute a long "intermediate cicatrix," and thus, by the formation of a piece let into the side of the urethra, as it were, produce a permanently enlarged canal. This operation M. Reybard appears to have first practised upon dogs, in which he had previously artificially produced a condition which he calls stricture, by wounding or cauterizing the urethra; after a suitable time he performed his operation upon them, and after another interval killed and dissected them, in order to observe the production of the "intermediate cicatrix."

The instrument which M. Reybard employs presents no kind of novelty in the principles of its construction, as compared with one of those used by Stafford. In form and some of its minor details it is modified. Thus, the blade is much larger, on account of the much greater depth of incision it is required to make. A dilating apparatus is also attached to its sides, consisting of two thin flat rods of spring-tempered steel, which can be made to project and dilate the urethra by a very ingenious mechanism, in order to stretch the mucous membrane before commencing the incision, and so ensure its complete accomplishment, in case the contraction is not sufficiently narrow to grasp and steady the instrument in its place! The size of the instrument equals No. 9 of our scale, but the action of the appended dilating rods is capable of increasing this enormously. Altogether the apparatus is a remarkable specimen of ingenious mechanical contrivance and finished workmanship, which no description, much less the very rough drawing of it in M. Reybard's work, can convey. A specimen of it lying before us at this moment, by the well known Charrière, of Paris, must be regarded as one of the *chef-d'œuvres* of that celebrated instrument maker.

M. Reybard admits the occurrence in his practice of very serious accidents consecutive to his operation. He describes a case in which he very nearly lost a patient from bleeding into the bladder. Hæmorrhage to a considerable extent he regards as a frequent and natural consequence of the operation. It will be considered in England that this is not a circumstance very unlikely to happen, since he advises the section of the contracted part to be directed towards either *side* of the urethra, and not in the median line along the floor, in order to avoid cutting the artery of the bulb (p. 385). In this country we lay great stress on the importance of the exactly contrary practice of cutting *in the median line in order to avoid those arteries*, and find it tolerably successful. M. Reybard's appreciation of the situation of the arteries in question has excited not a little astonishment in some quarters in this country, as may perhaps be already known to some of our readers.* Infiltration of urine he regards as another accident which may occur, although it has not happened more than once in his practice. In order to avoid it, a gum catheter is to be tied in for the first forty-eight hours. Violent febrile attacks are not infrequent consequences of the operation, but he denies that any objection arises out of the fact, because they are known to occur also after caute-

* The manifest anatomical error committed by M. Reybard in relation to the situation of the arteries to the bulb, unnoticed as it was by the Commission of the Imperial Academy, was pointed out by Mr. Syme, in a letter to that body, which communication, although acknowledged and referred to a special commission for consideration, was ultimately allowed to pass without reply.

riation, and even sometimes after simple dilatation; adding, that he has only lost one case in that way. Considerable infiltration of blood into the cellular tissue of the penis and scrotum, causing great enlargement, has occurred in a considerable number of cases, so much so as to have been regarded by the author at one time as the necessary and characteristic sign that a sufficient division of the urethra had been made!

The manipulations necessary for the performance of this operation, in such a manner as to fulfil accurately the indications necessary to its success, are said not to be easy, and a recommendation is given that they should be carefully practised several times upon the dead body before attempting them on the living subject. When a stricture presents itself which defies the skill of the surgeon to pass any instrument through it, M. Reybard proposes to puncture it a few times with an instrument from which a lancet can be made to project at the extremity, precisely after the first method of Stafford, and afterwards, when overcome and sufficiently dilated, to apply the urethrotome in the ordinary manner. This last proceeding—the mode of treatment, in short, which the author proposes to apply to those forms of the disease which constitute the really difficult cases—it surely will be unnecessary seriously to discuss, as it was long ago practised and since exploded here.

In endeavouring to arrive at a fair appreciation of this mode of treatment, claiming, as it does, to mark an era in the history of the therapeutics of this often very obstinate and serious complaint, and to be the only mode yet discovered of curing organic stricture, we must first inquire what are the essentials of the proceeding so characterized; in what respect they differ from those of operative measures hitherto employed; and lastly, whether our pretty extensive experience of stricture in this country permits us to acquiesce in the belief, that any single method of treatment at present known, can fairly lay claim to be the only curative one, to the exclusion of all others.

First, the essentials of M. Reybard's proceeding are, (a) The production of a long incision through the strictured part of the urethra, and the adjacent healthy parts, made in a direction from within outwards, and within the urethral canal, with no external wound, but sufficiently deep to divide thoroughly all the morbid tissue constituting the stricture; an incision which may err in not being sufficiently free, but can scarcely err, according to its author, in the opposite direction. (b) The production of a granulating wound by mechanically opening up the incision twice a day, so as to ensure the formation of the intermediate cicatrix, and thus enlarge the urethra permanently, to an extent corresponding with the breadth of the fibroid tissue which has intervened between the margins of the wound, as the result of the granulating process. This proceeding differs from all operations by internal incision of the urethra previously adopted, in the extent both as regards length and depth which characterizes the cutting operation in which it commences. It differs in its after-treatment, both in rationale and practice, inasmuch as it aims at the formation of the cicatrix described.

But, secondly, the complete division of the most inveterate forms of stricture by the knife, although quite permeable to instruments, has nothing of novelty in it for British surgeons, albeit it has hitherto been

foreign, except by report, to France. Mr. Syme, as nearly all the world might know by this time, has practised and recommended complete division in the cases denoted, although, not like M. Reybard, as an application for all forms and degrees of the complaint. He has, for reasons identical with those adduced by M. Reybard, advised the employment of a moderate amount of dilatation after the operation, on the express ground that union of the wound by the first intention does not produce such favourable after-results as the slower process of union by granulation. So far the principles are identical, but the great distinction between the two proceedings consists in this:—M. Reybard works in the dark, and makes incisions within the urethra visible to no mortal eye. Mr. Syme, cutting from the surface inwards, performs a free division of the stricture; and having guarded the surfaces of the wound from contact with urine during forty-eight hours, by a catheter tied in, encourages the urine, as well as the products of action in the wound, the purulent secretion, &c., to pass freely off through it—a method which ensures the desired union by granulation, and saves the thirty or forty days of dilating, to say nothing of the occasional introduction of a hooked instrument ‘like a lithotrite,’ invented and employed by M. Reybard for opening up the wound effectually, by driving the beak into it, when mere dilatation fails to keep the margins asunder. (p. 386.)

Will any one designate M. Reybard’s proceeding the less considerable, the less difficult, or the less dangerous of the two operations on *primæ facie* grounds? Probably no one in this country. Not so, however, M. Robert, the reporter of the Commission to the French Academy. In slightly adverting to the operation of Mr. Syme, in his report on M. Reybard’s paper, he represents it as the more serious, because in it “division of the soft parts in the perineum” is practised. But, provided that this incision lies wholly in front of the deep perineal fascia, from what source is the additional danger to arise, the urethra being freely divided in both cases? Do we not constantly inflict wounds of this character in the perineum in cases of extravasation of urine, and thereby save life, by permitting the escape of pent-up matter from the vicinity of a damaged urethra, the dangerous result which we always most fear when lesion of the urethral walls has been produced, unless a free external opening also exists—this being the very danger, moreover, to which M. Reybard’s procedure most exposes us? I conceive it must be admitted that the internal incision of M. Reybard is considerably more formidable than the external incision of Mr. Syme, on two accounts—first, because its extent (about three inches) is so great, that for strictures situated near to the posterior limit of the bulb, the deep fascia must inevitably be divided; and, secondly, because the necessary confinement of the products of inflammation, augmented as they are by the frequent introduction of various instruments subsequently, involves a state of greater hazard than one in which there is a free channel of escape not only for them, but also for the urinary secretion.

As to the question of whether or no stricture can be cured by dilatation, it is one which scarcely requires discussion here. M. Reybard, in one part of his work, lays it down as an axiom, that if a stricture does not appear within “one or two months” after any treatment, the patient is

safe from relapse, and the treatment must be declared successful. (p. 484.) This remark, it is true, is intended to relate to the result of his own cases of urethrotomy, enabling him to report them "cured" at a very early period. But is not this general admission altogether fatal to his system, grounded as it is upon the alleged inadequacy of dilatation to cure? Indeed, it saves us the trouble of disputing that point; for the daily practice of every surgeon who has had much to do with stricture, will produce cases without number, in which the patient has exhibited no relapse, within one or two months after treatment by dilatation, whatever he may have subsequently done. But it is easy enough to adduce examples of even narrow stricture, which during years since the treatment by dilatation, have exhibited no tendency to return. We do not hesitate to say, that one or two months is a period by no means long enough to test the lasting value of any mode of treatment. A much more extended term is necessary, in order to decide whether the freedom from contraction which a patient has obtained by means of any operative proceeding, is really permanent or otherwise.

In bringing to a conclusion this brief notice of the work before us, we feel bound to repudiate in the strongest terms the invariable appeal to the knife which M. Reybard declares to be necessary for the cure of stricture. In respect of internal incisions generally, as performed by any of those modifications of the primitive instruments which have sprung into existence during late years, each presenting some exceedingly slight difference from its predecessor, in accordance with the taste and fancy of its advocate, the opinion is certainly gaining ground that such measures, when applied to strictures affecting the posterior part of the spongy portion of the urethra—their most usual situation—are either useless or hazardous; the former if the urethral walls are not freely cut, the latter if they are. If the stricture be so confirmed that it will not yield to dilatation when carefully applied, so as not to occasion or augment irritability of the urethra, it may be taken for granted, as a rule, that a slight notch will not facilitate its disappearance. If it be granted that an extended section of the urethra is required in order to effect that which other means have failed to accomplish, then that section which is unconnected with any external outlet, and permits the occurrence of internal hæmorrhage and infiltration, must be held to involve a risk we are not justified in encountering.

Notwithstanding the pretentious claim set up on behalf of M. Reybard's method, we are satisfied that the most experienced surgeons here would feel themselves extremely ill-provided for the treatment of stricture did they possess only his single remedy, in the form of a cutting instrument, so large as to require a urethra almost of the natural size for its reception. So dangerous, so unnecessary, even so futile an application do we consider it to be, that it is really difficult to deal seriously with a proposal to employ it. And we feel compelled to say that we have done so only because serious men, as we have seen, have seriously presented the matter to the profession.

At this point we may naturally turn to the short monograph of Mr. Syme. It requires but a slight acquaintance with it to learn that its

object is a special one—viz., to explain and advocate the peculiar mode of treatment which its author has adopted in obstinate forms of stricture. First issuing in a distinct volume in the year 1849, it appears now as a second edition, but is nearly re-written. In few words we will state in what Mr. Syme's method consists, as a good deal of misapprehension appears still to prevail respecting it, both at home and abroad.

As a rule, then, Mr. Syme, in common with surgeons generally in this country, treats stricture by simple dilatation, eschewing the use of caustics and internal incisions. So firmly persuaded is he of the efficiency of the catheter, that he believes there is no stricture, however narrow, which will not admit an instrument, provided it be sufficiently small, and be employed with care and patience. He therefore believes that it is wholly unnecessary to resort to the operation of dissecting through what have been termed impermeable strictures, preferring to insinuate a catheter rather than to employ a knife in these circumstances. Hence he disapproves of that operation usually termed "the perineal section," which has been frequently resorted to in this country for the last thirty-six years. Having proceeded to employ dilatation, if he finds that the stricture rapidly re-appears in spite of it, or that the process involves much constitutional disturbance, he prefers to divide freely the stricture from the perineum, upon a grooved director, performing the incisions in the median line, tying in a catheter for forty-eight hours, and subsequently passing it a few times at about weekly intervals. This proceeding, to which he gives the name of "external division," is stated by him, on the ground of an extensive experience, to be devoid of danger, and generally to be attended with a successful result, a conclusion with which our own experience of it leads us, without any hesitation, to coincide.

The observations which first attract our attention in the work before us, have reference to the old question of impermeable stricture. Setting aside actual obliteration, Mr. Syme simply affirms that when urine passes out by the urethra, a catheter of appropriate size may always, with sufficient care, dexterity, and perseverance, be sooner or later passed into the bladder. On this point we yield adhesion to the principle announced, regarding a belief in it as a valuable and important acquisition to the surgeon. No doubt but that the man who most trusts his catheter will handle it most successfully, while he who uses it under the abiding impression that the trocar or the scalpel may be always employed to expiate a failure, will not overcome difficulties which the former will surmount. We are therefore glad to learn what has been Mr. Syme's later experience in relation to difficult catheterism. He candidly tells us that since he has made his well-known statement respecting impermeability, he has been unable, in the cases of three patients, to pass an instrument of the smallest size from the external meatus to the bladder, by means of manipulation only. This admission exhibits a fact of great interest. Did he puncture the bladder, or dissect through the stricture, in these cases? By no means: let us hear his own account of the proceeding:—"Indeed, on three occasions—one in private, and two in public—I found it necessary to open the urethra anteriorly to the stricture, so as to obtain the assistance of a finger placed in the canal, to guide the point of the instrument." (p. 36.) And we subsequently find details

relating to a case in which the stricture being complicated with a false passage, it appeared impossible to insinuate the smallest instrument through the contracted channel, until, the urethra being opened immediately anterior to the stricture, the operator was enabled to guide onwards with his finger the point of the catheter through it (pp. 93-6). We are bound to regard the extreme paucity of exceptions as a striking confirmation of the rule announced by Mr. Syme.

There is another observation which it is impossible to pass over in silence, relating to the part of the urethra which is affected by stricture. The author has on various occasions questioned the accuracy of writers in general who have treated of this subject. It is quite certain that we are even now constantly hearing of "strictures in the membranous portion," and not long ago it was common to speak of them in the prostatic part, or even "at the neck of the bladder." Such remarks always rested upon fallacious impressions received by operators in the use of instruments upon the living subject, and not upon anatomical observations, which, it need hardly be said, can alone determine the question. Discussing the merits of his operation, Mr. Syme writes the following respecting this matter:

"The only sources of danger that can be attributed to the operation, are bleeding and extravasation of urine; and in order to estimate the importance due to them, it is necessary that the true position of strictures should be ascertained. If they existed in the prostatic or membranous portion of the canal, extensive incisions, involving the deep fascia of the perineum, would be requisite; and, accordingly, this has been made a serious objection to my proposal by writers who quote the authority of Sir B. Brodie and others to prove the occurrence of stricture behind the bulb. But the fact is, that the seat of contraction is never so far back, and may be positively limited to that portion of the urethra which extends from the bulb to the orifice. The ground upon which I make this statement is, that in all my experience I never found it necessary to cut further back than the bulbous portion, for the conveyance of a full-sized instrument into the bladder." (p. 41.)

Now, with a view to the elucidation of this question, we undertook, some few years ago, a careful and laborious examination of every preserved specimen of stricture in the museums of London, Edinburgh, and Paris, comprising above 300 preparations, the results of which were published in our work before referred to. This examination confirmed Mr. Syme's observation to a remarkable extent, so far as it related to the non-existence of prostatic stricture, and to the *extreme rarity* of the affection in the membranous portion; but three or four preparations there are in this metropolis, in which, beyond all doubt, it does exist in that situation. And this, as a pathological fact, however slight its importance, may not be altogether lost sight of or ignored.

Any addition to our knowledge of the circumstances which may have occasioned failure after the performance of the operation, is extremely acceptable. Subsequent contraction at the site of the incision has been attributed by Mr. Syme to various causes, for the most part preventable. Among these, he points out more forcibly than ever the undesirable influence of union by the first intention in the incisions which have divided the obstruction. Thus he says—

"The most obvious and certain causes of relapses would appear to be adhesion by the first intention, between the edges of the incision made through the strictured part, which must restore the state of matters that existed previously to the operation. It might be expected, indeed, that the stream of urine passing over the raw surface would effectually prevent any such occurrence; and so I believed must be the case, until taught a different lesson," &c. (p. 56.)

In closing this necessarily short notice of the two works before us, we will advert to a hint given at the outset, respecting the existence of *one common principle of action* as the basis of the two proceedings referred to. Differing as those two methods do in application, *toto cœlo*, each rests solely upon recognition of the same fact, one which, until recently, was almost everywhere unacknowledged or unknown. Both aim at one result; both are alike valueless, nay more, are mischievous and dangerous, if the following proposition is not now proven—viz., that there are some cases of stricture in which either the deposit occasioning the contraction is so considerable in extent or density, or is associated with so much irritability of the urethra or of the system at large, that mere dilatation does not remove, and sometimes scarcely palliates the complaint; and that in these cases a free division of the diseased structure, not permitted to unite by first intention, affords a fair probability certainly of greatly diminishing, and often of completely curing, the disease.

For such cases, and for such only, we advocate that mode of division which Mr. Syme has practised, as the safest and most efficient treatment which experience has yet brought to light.

Henry Thompson.

REVIEW VII.

1. *Medical Testimony and Evidence in Cases of Lunacy.* By THOMAS MAYO, M.D., F.R.S.—London, 1854.
2. *On Medico-Legal Evidence in Cases of Insanity.* Being the Third Lettsomian Lecture. By FORBES WINSLOW, M.D., D.C.L.—London, 1854. pp. 77.
3. *Tableau Analytique des Maladies Mentales à l'Usage des Jurisconsultes et des Médecins.* Par Dr. J. PARIGOT, Professeur Hon. de l'Université de Bruxelles, &c.—Gand, 1854. 12 t. Folio.
Analytical Table of Mental Diseases, for the Use of Lawyers and Medical Men. By Dr. J. PARIGOT, &c.

THE true relation existing between crime and insanity is a question of the deepest importance to every member of every society in which laws exist and in which crime is possible; for every man is liable to become the subject of insanity, and thereby to become a criminal. A slip of the foot causing a blow on the head, an attack of fever with cerebral symptoms, and many other of the commonest accidents of life, may not only upset the balance of the stoutest minds, but may result in placing the most innocent and conscientious of men in the position of a criminal before his fellows. Dr. Johnson said that the saddest and most humiliating subject of thought was the uncertain tenure of the possession of reason; and the truth of this becomes more evident when we reflect, that when reason is gone there is no tragic act of which we may not become

the authors; that every man capable of reflection must recognise as a possible event of his life that he may imbrue his hands in the blood of those whose life is dearer to him than his own.

The interest and importance of this question is greatly augmented by the nature and magnitude of the crimes which arise from insanity. Murders of the most fearful and unnatural kind, wholesale domestic slaughters, assassinations and attempts to destroy the kings and rulers of men, these are crimes which result more frequently from insanity than from any other cause. To what extent the wide-spread desolations which have resulted from the crimes of kings may justly be attributed to criminal insanity, we shall never know. The motives of powerful men, and especially of despotic rulers, are too obscure to be fathomed even by observers who are close to the event in time and place; and when we attempt to appreciate those which have become matter of history, we can only feel that—

“The times that are gone by
Are a mysterious book, sealed with seven seals.”

The great ones of the earth have been peculiarly liable to insanity, from the time when Nebuchadnezzar presented the first instance of *zoanthropy*, to the days when Russia was overrun by the Swedish madman; even to the present time, when Europe is afflicted and affrighted by the gloomy and fanatical successors of him by whom the mad Swede was overthrown. The account has yet to be written of that criminal insanity in high places, whose overt acts have been wars and the desolation of nations. Our present inquiries take a humbler range.

We shall commence our analysis of these works with that of Dr. Mayo, an attention to which it is entitled, if not on account of the priority of its date of publication, at least and without dispute by reason of the antiquity of its opinions. M. Comte has shown that in all sciences, after the rude and barbarous period when men attributed all natural phenomena to the direct influence of deities,—when the winds were unchained, the storm aroused, or the thunderbolt darted, by the hands of Æolus, Neptune, or Jove, or by their representatives,—and when madness was attributed to the anger of the gods in general,—there is always a transitional period before men arrive at that of positive science, in which natural phenomena are attributed to metaphysical causes. This period, which M. Comte designates the metaphysical, is one of essences and immaterialities. It is in this period of pathological science in which at the present time we find Dr. Mayo.

“In respect to the actual changes which may be worked in the human being under insanity, it must be remembered by those who engage in this arduous inquiry, that they must be contemplated as concerning our immaterial phase of being in a light much more exclusive of that afforded by physiological considerations than we are accustomed to assume. That there should be a *disease of the mind in the abstract*, that such diseases should work changes in us, viewed in this light, analogous to the physical changes of our bodily organs, is neither unnatural nor inconceivable. A *parasitical growth*—if, for want of a proper term, I may borrow this epithet from physical speculation—may take place under such disease, itself possessing vital functions and energies, but having no other relation to matter than the obvious one on which the tenure of our present life is based, namely, that we have an immaterial and a material being indissolubly bound together for the duration

of that life; while, for anything we know, the immaterial element may be just as subject to its proper affections as the material one is. The above remarks may, at least, have a wholesome tendency to keep before us in our speculations the immense fund of mental disease that may exist, inappreciable through any knowledge that we at present possess of phenomena so little capable of being made the subject of experiment, or even observation, as those I am supposing."

This theory is beyond the pale of scientific argument. At least, it is beyond our reach, and must enjoy the usual immunities of incomprehensible metaphysics:—Disease of the mind in the abstract; a parasitical growth of our immaterial existence, that which has neither length, breadth, nor substance; covered with lichens like an old apple-tree, or swarming with objectionable vermin like a dirty beggar! To what indignities may not the immaterial soul be subjected!

But granting to Dr. Mayo the undisturbed possession of his theory, that insanity is an affection of the "immaterial phase" of our being, and that it is a disease of the mind in the abstract, have we not a right to inquire in what manner he will distinguish this affection from that condition of the mind which produces crime? Granting the immaterial nature of the mind, and that insanity is a disease thereof, parasitical or otherwise; what is vice? what is wickedness? from what fount in the caverns of the soul does the dark stream of crime arise?

In Germany, men who are called physicians, but who have been content to live in the dreamland of speculation rather than in the active work-a-day world of reality and of phenomena, have entertained and maintained the same opinion of the nature of insanity as Dr. Mayo; and they have not hesitated to adopt the legitimate consequences of that opinion. The school of Heinroth maintains that insanity is identical with spiritual sin, and that of Ideler teaches that it is mainly owing to moral depravity; a verbal difference appearing to exist between the two.* Nor have these opinions been confined to the closet. They have found expression in the lamentable harshness with which lunatics are still treated in Germany. The reckless application of punishment to the insane, both in prisons and in asylums, can find its only justification in such detestable theories.

It is easy to trace throughout the whole of these lectures the effect of this opinion in removing the insane from that pity and full sympathy which Dr. Mayo would doubtless feel for their unhappy state did he believe that, like other diseases, it was referrible to organic affection only. For example, writing of the "mischief occasioned by the doctrines of

* The principles of the different psychological sects which now divide the medical profession in Germany, is well and concisely expressed in the following extract from the May number of Damerow's 'Zeitschrift für Psychiatrie.' "Den *Ursprung* der Seelenstörungen betreffend, so ist auch hier noch immer keine Uebereinstimmung erzielt worden und namentlich stehen zwei feindliche Lager sich noch immer sehr schroff gegenüber; ich möchte sie nach ihren Grundanschauungen die *Somatiker* und die *Moralisten* nennen. Während die meisten Irrenärzte die Ueberzeugung von dem körperlichen Ursprünge der Seelenstörungen festhalten, hält ein viel kleinerer Theil noch immer an der Ansicht fest, die Grundursache derselben liege in geistigen und moralischen Einflüssen. Die Hauptträger dieses Idenganges, *Heinroth*, und *Ideler*, weichen nur darin von einander ab, dass Jener, Ursprung und Wesen der Insania in allgemeiner *Sündhaftigkeit* und *Lasterhaftigkeit*; Dieser, in einem übermäßigen Vorhandensein von *Lebensschaffen* sucht. Bei den Somatikern gehen die Meinungen nur darin auseinander, dass man nicht darüber einig werden konnte, ob das Gehirn, als das eigentliche Denkkorgan, immer und ausschliesslich der körperliche Sitz und Ursprung der Krankheit sei, oder ob diese auch direct durch ein körperliches Leiden aller andern Organe erzeugt werden könne."

moral insanity" on account of the "impunity it affords to crime," he states, "No such impunity is implied in the hypothesis which avowedly represents *the abnormal state as a mode of wickedness, consisting in the non-development or absence of the moral sense.*"

When Dr. Mayo sees the lengths to which he is bound to follow his opinion that insanity is a disease of mind in the abstract, we trust he will reconsider the grounds of his belief, even if such a step should lead him to desert the speculators, and to enlist himself in the ranks of those who are content to reason alone, upon matters which are capable of being observed.

Dr. Mayo would punish for crimes committed during a supposed lucid interval. Quoting from Rae, that we ought never to convict for a crime committed during that time, because there would be every probability that the individual was under that cerebral irritation which makes a man insane, he says—

"That this law is not conformable with the dicta of the judges will not be disputed if my interpretation of those dicta is accepted. That it is not more conformable with the interests of society will also, I think, be admitted."

But it would be a thankless and unprofitable task to pick out all the expressions which bear the stamp of Dr. Mayo's opinion of the radical nature of insanity. They are such as would justly be deemed harsh, if they proceeded from a person who saw in a lunatic an afflicted but innocent sufferer from disease of the brain; but emanating from one who thinks insanity "a disease of the abstract mind," who confounds "abnormal states" of the mind with "modes of wickedness," they must be accepted as the imperfect results of a theory which a benevolent disposition alone can prevent the reason from pursuing to its logical and cruel last consequences.

Has Dr. Mayo ever reflected from whence he derives his right to express an opinion respecting the nature of insanity, or to undertake its treatment? When, a few weeks ago, he stood up in a court of justice and pronounced with authority his belief that Luigi Buranelli was not insane, did he understand from whence he derived his mission to do so? If the opinion was common in this country that insanity is a disease of the abstract mind, neither to Dr. Mayo nor to any other physician would it be permitted to claim any right to pronounce with authority an opinion thereupon. Insanity would be taken from the domain of medicine and given over to the mental philosophers; or if in this practical common-sense country a sufficient number of such dreamers was not to be found, one step further back, would consign the lunatic to his old guardian, the priest.

If Dr. Mayo's opinions are not unsound, the pathologist and the physician have no more to do with insanity than with the origin of evil or the nature of the beautiful. Let those who have the care of the insane dismiss Andral and Rokitansky to the dusty oblivion of the upper shelves, and make Aristotle and Plato, Kant and Hegel, their instructors and guides. But the practice common in this country contradicts these views, and places any man who may entertain them in complete disaccord with the prevailing tendency of thought.

Respecting the marks by which the presence of insanity may be known, Dr. Mayo says: •

“In looking for a term which may contain the essential elements of insanity, and therefore confer a criterion of its presence, I adopt delirium, as used by M. Pinel and Dr. Cullen. . . . Delirium is generally contemplated as involving two conditions in this sense—that one of them must be present in every delirious person, both being generally present. The first of these conditions is a negative one. It is the absence of that control over the rise and succession of thoughts which exist in the same mind, and co-existent with this, a state of ill-associated and incoherent thought. . . . The second phase of delirium consists of the presence of certain delusions or false perceptions, of which there are two principal forms—namely, those of the special senses, which are called objective, and those having no reference to objects of sense, but turning on perceptions of the understanding alone.”

Why Dr. Mayo prefers to call this latter variety by the quaint term of “notional delirium,” we cannot tell. The converse of that which is objective is subjective; while idea supplies the converse of notion. But it would be vain to expect precision in the use of terms from an author who, on the supposed authority of an interpretation clause, of an Act of Parliament, draws a wide distinction between insanity and unsoundness of mind—terms which are precise synonymes, notwithstanding that lawyers have more affected the use of the Latin derivative in criminal cases, and that of the Saxon in civil procedure.

But is *delirium* an “essential element of insanity” and “a criterion of its presence”? If so, we have to unlearn all that has been added to the science of mental pathology, from the time of Pinel and Cullen to the present. If we give up the *Manie sans Délire* of Georget, the *Monomanie Homicide* of Esquirol, the Moral and Instinctive Insanity of Pritchard, what shall we say of the numerous cases of Melancholia in which there is neither incoherent thought nor delusion? What shall we say of those fearful cases of suicidal impulse, in which the intellectual faculties are intact, while the instinct of self-preservation has undergone the most complete perversion? As well might Dr. Mayo assert that delirium is the essential element, the true criterion of the presence of fever.

We have devoted more space to the examination of Dr. Mayo's work, than we should have felt ourselves justified in doing, had not its contents received a certain stamp of authority by passing to the public through the lecture hall of the Royal College of Physicians. We trust that the next time the London College of Physicians submits to be lectured to on the subject of insanity, it will be more fortunate in the selection of its instructor. One more antiquated in his notions, more unprecise in his use of terms, and more illogical in his argumentation, it would be difficult to find than the Croonian Lecturer of 1853. Moreover, whatever practical acquaintance Dr. Mayo may have had with the phenomena of insanity must have been lost upon him, because he has viewed them as symbols of disease of the mind in the abstract, too unimportant to attract particular attention; for of the essential elements and the true criteria of cerebro-mental disease, these lectures scarcely contain a notice.

Dr. Forbes Winslow's lecture “On Medico-legal Evidence in Cases of Insanity,” delivered before the Medical Society in London, is a lecture indeed. It occupies seventy-seven pages of closely-printed royal octavo, and although the lucidity of its argument, the elegance of its language, and the absorbing interest of its subject matter, must have rendered it

impossible for the audience to have become inattentive or weary, still we fear that the learned author may have something to answer for, by having afforded the occasion of overwrought mind among not a few of his professional brethren.

We shall commence our examination of it by a quotation in which the author sets forth his views of the ultimate nature of insanity. Opinions on subjects of this abstruse nature have a more direct bearing upon medical and legal practice than would at first sight appear probable. For instance, Lord Brougham ignores all forms of partial insanity on account of his metaphysical views respecting the nature of the mind, which he believes to be one, indivisible, and without parts. The mind has neither an upside nor a downside, an inside nor an outside, a right side nor a wrong side. It has no limits or boundaries, and is therefore diffused through space. It has no parts, even of an atomic nature. It is imponderable and immensurable, and is therefore totally unlike any gaseous body; which latter is indeed as truly material as muscle or cerebral substance. There are no real distinctions in the operations of this spiritual and essential unity. Thought is like feeling, feeling like instinct, and instinct like thought. They are different modes of the same essence; as in the modes of fire, light is like heat, and heat is like smoke, and smoke is like light. Therefore no one can be partly insane and partly sane. Monomania is a delusion founded upon the supposed but unreal distinctions of mental function. According to this noble and learned metaphysician, there is, and there can be, no such thing as partial insanity—a conclusion which has succeeded in convincing us, not that our senses have been unfaithful in recording upon our memory a great number of such cases, but of the extreme danger and absurdity of applying the “*à priori*” mode of argumentation to matters of science. The intellectual function, or mode of mind, submits itself to examination much more easily than the instinctive or the emotional. If, upon examination, the intellectual mode was found to be unaffected by disease, the entire mind must, upon this theory of mind, be pronounced sound. For it must be remembered that assent to the proposition, founded upon the Unity of Mind theory, “Prove one function of the mind unsound, and the whole must be unsound,” carries with it, as a logical sequence, assent to the converse proposition, “Prove one function of the mind sound, and the whole must be sound,”—a proposition which, if acted upon, would conduct many a madman to the scaffold.

The views of Dr. Forbes Winslow upon the ultimate nature of insanity are most explicitly set forth in the following passage:

“We observe the principle of vitality manifested through different physical media; but whatever may be the character of the material tissue, or the special function of the organic structure through which life reflects its powers, we, as *Spiritual Physiologists*, maintain that these manifestations are only different modes or states of development of one and the same principle; that the life that manifests itself through the brain, lungs, stomach, or the heart, is identical and homogeneous in its nature and essence: the peculiarity of the physical organization affecting, as it undoubtedly does, its mode of being action. Applying this metaphysical doctrine to the subject now under consideration, it must be evident that in all the varied phenomena of insanity the same identical essence or principle

is affected; that without any exceptions, the Mind—using this term in its liberal and philosophical acceptation—is in a state of disorder. I would, however, protect myself from repudiating the great discovery of Gall, or of holding with the spiritualists, that the principle of thought is susceptible of actual disease, apart from any abnormal state of the cerebral tissue. In all cases of mental derangement, the *manifestations* of the mind, and not the mind itself, are implicated; or, to speak with a strict regard to the principles of cerebral pathology, the physical media, or the different portions of nervous matter through which the intellect is developed, are diseased, and as a necessary consequence, the principle of thought is disordered or deranged in its operations.” (p. 119.)

Thus it appears that Dr. Winslow also considers insanity to be a disease of the mind in the abstract, but he differs from Dr. Mayo in adding thereto an abnormal state of the brain as a *tertium quid*. He thinks that in insanity, without any exceptions, the essence or principle of mind is affected; but he does not deny that in combination with this affection of the essence of mind, there must be an abnormal state of the cerebral tissue. We do not clearly understand what he means by “the manifestations of mind, and not the mind itself, being implicated in all cases of mental derangement,” unless he refers to the mind itself as an ontological entity distinct from its qualities. Granting, for the sake of argument, the existence of an entity of this nature, we must observe that it appears impossible to prove, and most difficult even to conceive, any change taking place in the qualities or manifestations thereof, without change in the thing itself. In the above paragraph, Dr. Winslow avows himself to be a *spiritual physiologist, and also a materialist of the phrenological school*. With the mental chemistry which unites these apparently incompatible elements, we are unacquainted. To our understanding, it appears a mixture rather than a combination. But as Dr. Winslow maintains that insanity cannot occur from an affection of the principle of thought, apart from abnormal states of the cerebral tissue, and in fact, as he states in several other passages, that insanity is always accompanied by disease of the brain, we gladly accept the latter half of the author’s opinions on this matter as the practical basis of his judgment in cases of criminal lunacy.

We have seen that the spiritualist doctrine, comprising that of mind being “an essence one and indivisible,” (p. 118,) is inconsistent with the theory of partial insanity; and if Dr. Winslow adhered to the metaphysical opinions he has enunciated on this point, he would find it difficult to discover a logical route to the various forms of partial insanity which he recognises, or to the degrees of insanity and responsibility, which, we are happy to say, he not only admits, but on account of which he eloquently pleads for a *system of graduated and mitigated punishments*.

“As the plea of insanity is one of the most important that can be urged in a court of justice in extenuation of crime, it should never be had recourse to except in clear and obvious cases, in which little or no doubt can be entertained, not only of the existence of mental derangement, but of derangement of such a *kind*, and to such a *degree*, as to justify the immediate admission of the fact, and the necessary and consequent acquittal of the prisoner.” (p. 109.)

“We should never forget in many criminal cases the alliance to insanity is close—the line of demarcation between the two conditions indistinct, vague, and shadowy, the boundary separating crime from insanity obscure—the one state

often, almost imperceptibly, blending with the other, and that the facts associated with the criminal act so analogous to the recognised phenomena of mental disease." (p. 155.)

"I maintain, and facts—an overwhelming mass of facts—clearly, irresistibly, and conclusively demonstrate my position, that there is a vast amount of crime committed by persons who, if not 'legally,' or 'medically,' insane, occupy a kind of *neutral ground between positive derangement and mental sanity*. I do not broach this idea with the view of supporting the absurd, unphilosophical, and dangerous opinion, that all crime is more or less referable to aberration of mind; but I do affirm, that in estimating the amount of punishment to be awarded, it is the solemn duty of the judge, not only to look at the *act itself*, but to consider the *physical* condition of the culprit—his education, moral advantages, prior social position, his early training, the temptations to which he has been exposed, and, above all, whether he has not sprung from intemperate, insane, idiotic, and criminal parents." (p. 156.)

On referring to Dr. Winslow's earlier works, we find that this philosophic and truly humane opinion has been long and consistently maintained by him. His work on the 'Plea of Insanity' concludes with the following sentence:

"I again repeat, that I am not prepared to give an unqualified assent to the dogma, that in every case of mental derangement, without any reference to its degree or character, ought the person to be screened from the penalty awarded by the laws for criminal offences. I am ready to admit, that if insanity be clearly established to exist, a *prima facie* case is made out in favour of the prisoner; but that because a person may be proved to be strange and wayward in his character, to fancy himself a beggar when he may have the wealth of a Cræsus, or to be ill when he is in the buoyancy of health—to believe that such a person ought of necessity to be exonerated from all responsibility, is a doctrine as unphilosophical and untenable, as it is opposed to the safety and well-being of society." (*Plea of Insanity*, p. 78.)

Dr. Winslow himself observes on this passage:

"Our opportunities of extended experience have been great since the publication of this opinion. The more we have seen of insanity, particularly among criminals, the stronger are our convictions that this is the sound, the safe, the philosophic view of derangement of mind complicated with crime. If the extreme view of the subject is recognised and acted upon, society would not be safe; if every shade of disturbed mind, if every amount of eccentricity, even when evidently associated with unhealthy cerebral organization, or even incipient insanity, is to shield persons against the just punishment awarded for offences against life and property, we might be justified in closing the doors of our criminal courts, and superannuating the judges who are entrusted with the administration of the laws! If we carried out this principle to its full extent, many great criminals would clude the hands of justice, and escape the just punishment awarded for crime." (*Psychological Journal*, vol. iii. p. 448.)

He elsewhere laments the want for such cases "of establishments between a prison and a mad-house."

In his remarks upon the trial of Robert Pate,* Dr. Winslow observes—

"We propose to make it compulsory for the jury always to assign the motives of a recommendation to mercy. If such a rule obtained, what more valid motive could a jury adduce than that of a doubt respecting the perfect sanity of the prisoner? Cases are continually occurring in which the evidence, though insufficient to warrant an acquittal on the ground of insanity, is conclusive as to the

* *Psychological Journal*, vol. iii. p. 456.

existence of such an amount of mental derangement as should serve to extenuate the prisoner's guilt and mitigate his punishment.

"When the unsoundness of mind is found to amount to what the law terms total insanity, we would retain the present form of verdict; but in those cases of doubtful character, in which proof of some previous attack of insanity, or habitual eccentricity, or recent change of sentiment and conduct, not amounting to evidence of positive lunacy, and having no immediate connexion with the crime, yet sufficient to suggest the suspicion of so much mental unsoundness as would tend to impair the will, obscure the judgment, and preclude a full, clear, and just apprehension of the criminality of an offence; in such cases we would authorize the jury to return a verdict of guilty, with a recommendation to mercy on the ground of presumable insanity." (p. 123.)

In an article on the Plea of Insanity in the same journal (No. 17), he urges the same opinion, and suggests a remedy.

"In every criminal case where the question of responsibility arises in the course of judicial inquiry, if it be possible to establish any degree of positive insanity, it should always be received as a valid plea for a considerable *mitigation of punishment*, and as *prima facie* evidence in favour of the prisoner; and in no case where insanity clearly exists (without regard to its nature and amount) ought the extreme penalty of the law to be inflicted.

"In its present state, the law permits no discrimination; for so far as punishment is concerned, it takes no account of the degree of insanity, and recognises no intermediate condition between perfect sanity and total insanity; but the change we have proposed would satisfy the necessities of the case, and render the law more consonant with the enlarged humanity and progressive enlightenment of the age."

We have been thus careful to place the opinions of this able psychological writer on this question fully before our readers, because it appears to us at the present time to be a most important one, urgently demanding a practical solution. We have ourselves on several occasions maintained, to the best of our ability, and with the earnestness of strong conviction, opinions exactly tallying with those so forcibly enunciated in the passages we have quoted from Dr. Winslow. They have not been allowed to pass unchallenged; but we venture to affirm that they are incapable of refutation, unless the origin of insanity in disease of the brain can be proved to be an error. If insanity is an affection of the mind, as distinguished from the brain, and if the mind is an essence one and indivisible, we give up as untenable the doctrine of its partial disease. That which has no parts cannot be diseased in part. But if the cause of insanity is to be sought in bodily changes, we know, as physicians, that we have firm and trustworthy ground under foot, and with the force of experience derived from a vast mass of recorded instances of the special disease in question, and also from general principles of pathology, we know that the degrees and varieties of this disorder are and must be infinite. If this be true, degrees of insanity exist, for which exemption from punishment cannot be claimed, unless it be maintained that the very lowest amount of insanity ought to confer immunity from all responsibility, a proposition perfectly untenable. It is, however, such a proposition that the opponents of modified responsibility are compelled tacitly to adopt. They cry, Punish a madman! what monstrous cruelty! forgetting that modified punishment may be, and often is, an act of the greatest kindness and mercy to the individual punished. It is not, and it never has been, the law

of this country, that all the pale and faint shades of mental disorder should shield the doer of a crime from the consequences of his act. From the times of Coke and Hale to the present day, judges have busied themselves in defining the kinds and degrees of mental disturbance which should be deemed sufficient to excuse the commission of crime. But the opponents of mitigated punishment ignore this fact, and argue upon the assumption, that any trifling amount of deviation from the type of healthy mind can reasonably and legally be urged as a complete bar to punishment. The fractious and peevish subject of gout,* in whose blood Dr. Garrod had found some lithic acid, would possess, according to these reasoners, an equal immunity for acts of violence with the man who was urged to acts of delirious fury by a more potent blood-poison—that of typhus, for example, perverting all the cerebral functions. It is easy to see the logical fallacy into which these writers have fallen. The immunities justly accorded to mental disorders of a certain kind and degree, they claim for all disorders of the mind whatsoever. It is the everlastingly recurring fallacy which is effected by changing the premises, *à dicto secundum quid, ad dictum simpliciter*. We have been astonished at the vehemence and pertinacity with which this flimsy argument has recently been urged. But for this and for the continuance of practices consistent with this false theory, but utterly opposed to the fundamental principles of justice, we should have scarcely deemed its refutation worthy of the ink we have shed upon it. If the necessity and justice of mitigated punishments founded upon the theory of responsibility being modified, but not entirely removed, by low degrees of mental disorder, had been recognised in the practice of our courts of law, we entertain the strongest conviction that one of the most appalling crimes on record—that of the adulteress-murderess, Mary Ann Brough—would not have been declared free from guilt; nor would the poor Italian, Buranelli (the evidence of whose insanity was infinitely stronger than that which existed in Mrs. Brough's case), have been made to expiate his deed of frantic revenge upon the scaffold. In these cases of diminished responsibility, the pendulum of judicial decision oscillates between the mischievous example of crime declared to be free from guilt, and the piteous spectacle of a wretched being subjected to the extreme penalty of the law; in spite of a reasonable belief that he was the subject of mental disease, however slight in degree, or unconnected with the act for which he suffers.

Dr. Winslow has not only pointed out with great clearness the true doctrine of modified responsibility and mitigated punishment in cases of partial insanity—he has suggested the reforms most obviously needful to carry these sound and just opinions into effect. He especially recommends that the jury should have the power to return a verdict in three forms—namely, the two to which they are now restricted—*guilty*, or *not guilty, on the ground of insanity*, and also an intermediate one, *guilty, but recommended to mercy on the ground of presumable insanity*; according to which three forms, he proposes that the judge should be compelled to regulate his sentence; thus assimilating the rule of procedure in England to that which obtains in France, in the use of the verdict of *Guilty*;

• * See *Psychological Inquiries*, p. 30.

with extenuating circumstances. Establishments "intermediate between a prison and a mad-house," the want of which Dr. Winslow so much laments, and which, we apprehend, would somewhat resemble the Asylum for Lunatics of Criminal Disposition which we have ourselves urgently recommended, would enable the judge to award a modified sentence in the manner most beneficial to the criminal.

Should any other changes in legal procedure be found needful to carry into successful practice the great principle of punishments modified according to degrees of responsibility, we are assured that Dr. Winslow will urge their adoption, notwithstanding the opposition which such changes would meet with from those, who reverence the laws of England for their antiquity rather than on account of their justice and excellence. He is not one of those men who adhere with blind tenacity to that which is false and defective, merely because it has received the stamp of authority. The *liard*, with its coating of silver and its imperial N., is for him nothing more than a bit of brass; and he is the last man to appeal to the great charter in defence of the defective or bad customs of our courts, and to cry with indiscriminating zeal, "Nolumus leges Angliæ mutari." On the contrary, he thinks that the "first principles of law" have yet to be decided upon by our judges, and that, consequently, matters of procedure and custom founded upon such principles are unfixed, fluctuating, and reformable:

"Before this can be recognised as a safe standard, it will be necessary for British jurists to lay down for their *own* guidance certain fixed and unalterable principles of jurisprudence. Is it not a notorious fact that, on apparently clear and well-recognised points, lawyers of eminence have arrived at the most opposite conclusions? One court reverses the judgment of an inferior tribunal, and one distinguished jurist overrules the decision of his predecessor. As long as able judges differ among themselves upon what may be termed *first principles of law*, it will be unreasonable to expect that *we* should prostrate ourselves before the legal test which I have been analyzing." (p. 108.)

We are inclined to differ from Dr. Winslow in thinking that difficulties of this question have arisen, not so much from any defect or uncertainty in the first principles of law, as from the clumsy and imperfect method of admitting medical evidence and conducting the formalities, which legal custom has sanctioned for the elucidation of truth. The manner in which scientific evidence is hunted up and marshalled by attorneys in criminal lunacy trials, is indeed lamentable. It is degrading to physicians holding a high position in the science of cerebral pathology to be calculated upon and made use of by lawyers, as trump cards in the trial of forensic skill on one side or on the other. Dr. A. thinks all men mad, therefore he is always subpoenaed for the defence. Dr. B., on the contrary, thinks all men are wicked whose habits of thought and conduct are strange: he, therefore, is certain to appear as evidence on the side of the Crown. Alas! for the honour and dignity of the profession, when physicians of high standing and of spotless integrity can in this manner be used by any astute or unscrupulous attorney to get the chesnut out of the fire. Let it not be forgotten, that if the medical witness, in consequence of the evidence adduced, changes the opinion he was supposed to entertain before the trial, he is not called into the witness-box—he is passed over, *sub silentio*, as an unprofitable servant; and this is done not only in the defence, but on the

part of the prosecution. It is scarcely defensible in the former case, but in the latter it becomes an act of atrocious injustice, equivalent to the concealment of evidence which would prove the innocence of a man arraigned for a great crime. What would be thought of a prosecutor who, having brought a witness into court with the purpose of giving testimony that he had seen a murder committed by a person who was on trial for the deed, but who discovered that he had mistaken the identity of the prisoner whom he saw at the bar—what would be thought if, under such circumstances, the prosecutor withdrew that witness from the court, and withheld the testimony he was able to afford respecting the innocence of the prisoner?

The whole proceedings in our criminal courts for ascertaining the existence or non-existence of insanity are awkward, unscientific, and uncertain. They are such that it becomes in a great degree a matter of accident whether a guilty person escapes under the false plea of lunacy, or a lunatic is found guilty and hanged. They form a most painful and humiliating contrast to the scientific proceedings by which such questions are determined in France. We recommend such of our readers as may be desirous to see what medical evidence in cases of the plea of insanity ought to be, to turn to the pages of the 'Annales Médico-Psychologiques,' which excellent periodical contains numerous reports of the *experts* employed by the French judicature. These *experts* examine the supposed lunatics in the most painstaking manner, conversing with them day after day, observing them at all hours, confronting them with their friends or old associates, and even giving them chloroform to throw them off their guard. They make out the history of the whole life of the patient, and subject it to a strict analysis. Upon these data they make their report, being under oath; and we must say of these reports, that they are remarkable for sound reasoning, extensive psychopathic knowledge, and integrity of purpose. We greatly regret that their length prevents us from giving one of them as an example. It is an exceedingly rare occurrence for the court to differ from the conclusions arrived at by the *expert*. Compare this system with that which prevails in our courts, where an array of medical men are marshalled by the attorneys on each side, according to their preconceived opinions of the case. These medical witnesses may usually be divided into two classes—those who know something of the prisoner and nothing about insanity, and those who know something about insanity and nothing of the prisoner. They generally succeed in neutralizing each other's evidence, and in bringing the medical profession into contempt, at least among lawyers. One of the most experienced of medical witnesses (Professor Taylor) admits that the fate of the prisoner is, for the most part, a matter of chance—a proof that it is seldom determined by scientific evidence, for science and chance have no relation to each other. The French system, which is not an absurd plan of medical juries, but which places the scientific *expert* before the court in an independent and impartial position, and affords to him an ample opportunity to form a decided and trustworthy opinion, appears to be in every way worthy of imitation.

The medico-legal notes of Dr. Forbes Winslow on the case of Buranelli,

afford ample proof of the inveterate tendency existing among psychologists to see only one side of these difficult questions. He states :

“ If a *prima facie* case of mental derangement be established *in favour* of an accused person, the testimony of a scientific *expert*, although necessarily speculative, is legitimate and admissible. *His object is to save human life*, by affording the prisoner the benefit of any doubt that may have been raised as to his sanity and responsibility when the overt act of crime was committed. The witness may, with the best intentions, come to a *rash and unjustifiable conclusion*, and if such should be the case, *no serious injury* to society ensues if, as the result of his evidence, a fellow creature is rescued from the hands of the public executioner. On the other hand, if, in a criminal case, a medical witness incautiously or inadvertently gives a wrong opinion, a monstrous act of injustice and cruelty may be perpetrated, for which there can be no remedy. A scientific witness has no right, if called upon, to give such evidence, from the conviction that he cannot do so without recklessly trifling with human life. When we consider how suddenly symptoms of homicidal insanity develop themselves, how transient and evanescent these attacks are, that *a man may be wildly delirious and irresponsible in the morning, and sane, rational, and responsible in the afternoon*, how can a medical witness speak with satisfaction on the subject? If we were asked if Rush and the Mannings were of perfectly sane mind when they committed the brutal murders for which they justly suffered the extreme penalty of the law, we should certainly decline committing ourselves to an opinion, if the lives of these miserable criminals rested upon the answer we gave to the interrogatory.” (p. 64.)

These opinions are highly creditable to the benevolence of Dr. Winslow's disposition, but we fear that they will not be received with much favour by the administrators of justice. Every member of society is liable to be called upon, and to be compelled, to aid in the administration of the laws of society, however painful that duty may be to his sensibilities; and we apprehend that a medical witness, whose regard for the life of criminals led him systematically to adopt and candidly to avow the line of conduct advocated by Dr. Winslow, would expose himself to the stern rebuke of the court, and the rude remarks of the gentlemen of the bar, if not to consequences still more unpleasant. If such reluctance to aid in the punishment of criminals as that avowed by Dr. Winslow should become general, how would the duties of the jury-box be discharged?

With regard to the *nature of insanity*, we must express our own decided conviction that it depends solely and entirely upon disease of the brain. Insanity we have ourselves defined to be “ *a condition of the mind in which a false action of the conception or judgment, a defective power of the will, or an uncontrollable violence of the emotions and instincts, have separately or conjointly been produced by disease.*” This definition an able writer declares to be “ *superior for juridical purposes, as even a layman may pronounce, to those that have preceded it.*” * But supposing this to be the case, and the definition admitted as on the whole correct and satisfactory, how is any concrete instance to be recognised as falling within it? How are the conditions of mind which it indicates to be known as the product of disease, and therefore as insane conditions? In the classification of men into those who are sane and those who are insane, the only trustworthy mark is furnished by the absence or presence of that which is the *cause* of the difference — namely, *disease*. Causes are always preferable to any other mark in scientific classifica-

* Crime and its Excuses: Oxford Essays. 1855.

tion, "both as being the surest and most direct of marks, and as being themselves the properties on which it is of most use that our attention should be strongly fixed."* It is the more needful in this case to take the cause as the mark of the class, inasmuch as no one of the effects is in the slightest degree trustworthy as a mark of the whole class. Disease, therefore, of the brain must be accepted as the sole mark whereby insanity can be predicated; and the demonstration of its existence is the rightful, though oftentimes difficult, task of the physician.

In the accomplishment of this task one of two methods may be adopted. The phenomena of all forms and varieties of insanity, which have been ascertained to be such by accurate and faithful observation of their results, and by post-mortem examinations, may be recorded, and the varieties classified thereupon; so that when a new instance arises it may be referred by its phenomena to its proper place, in the same manner that a case of skin disease may be classified, even by a tyro, by reference to the descriptions and the atlases of Wilson or Rayer. This method, which is strictly experimental, has been recently adopted by the experienced Belgian physician whose work we have cited at the head of this article. Its application, however, to juridical purposes would appear to be difficult, if not impossible, for the phenomena recorded would never be admitted as a standard of comparison; and even were the admission made, the symptoms of insanity are so infinite in degree and character, that it would be far more perplexing to find, for a particular instance, its right place in this complicated classification, than to prove, deductively, its right to be so classified. Still, such systems have their use. They lead to the diligent and continuous observance of phenomena, without which, argumentation upon the basis of definitions is continually tending to become a war about words—a mere logomachy. "Words, however well-constructed originally, are always tending, like coins, to have their inscription worn off by passing from hand to hand; and the only possible mode of reviving it is to be ever stamping it afresh, by living in the habitual contemplation of the phenomena themselves, and not resting on our familiarity with the words which express them." †

A minute observance, and a painstaking classification of the phenomena of insanity, is therefore justly to be deemed a necessary foundation for the more difficult intellectual task of deciding whether the symptoms presented by a new case, of dubious nature, entitle it to be ranked in the category of insanity or not. This must be effected upon the broad principles of general pathology, and not by the aid of the minute distinctions of systematic nosology. This latter system—which may be called the deductive, in contradistinction to the experimental—is the one we have ourselves endeavoured to elucidate in the work quoted further on, and the principles of which may be said to depend upon a careful and philosophic comparison of the individual with his former self, and upon the relation of cause and effect in the influence of circumstances affecting the condition of the mind. Thus, if a circumstance which usually produces sorrow or chagrin is followed by extravagant spirits, and if such excitement, bidding defiance to advice and reason, gives way under the administration of pharmaceutical remedies and the influence of judicious

* Stuart Mill.

† Ibid.

control, there can be little or no doubt that the excitement has been caused by cerebral disease.

The etiology of disease must be allowed considerable weight in deciding upon difficult instances. Changes in the habits, or dispositions, or intellectual powers coming on after an injury to the head, or a fever, or intense mental excitement, or grief, will be more readily acknowledged as symptoms of mental disease than if they had been observed without the occurrence of such well known causes. The change which took place in the habits and character of Buranelli derived importance from the fact that they occurred shortly after, and apparently in consequence of, the death of a tenderly beloved wife.

The existence of any signs of disease of those portions of the brain which are not subservient to the mental functions, are of immense diagnostic value. It has occurred to us on more than one occasion to detect the early symptoms of general paralysis in persons accused of theft. The existence of epilepsy would, in our judgment, go far to prove that acts of violence were the result of cerebral disease. It is also well known that various forms of insanity often come on after attacks of cerebral hæmorrhage. But we must admit that it is quite necessary to be cautious in the admission of a past attack of cerebral disorder as a proof of insanity. Great stress was laid upon the puerperal convulsions from which Mary Anne Brough had suffered some years before the commission of her crimes. They had not left behind the slightest indication that the brain was permanently injured, and it would have been difficult to prove that they had a more intimate connexion with her crimes than might have been exerted by an attack of teething convulsions during the more remote period of infancy. To admit that proof of any previous affection of the sensory motor system afforded sufficient ground for inferring the existence of mental disease, would be extremely unphilosophical and unsafe.

The different forms of insanity dependent upon the particular function of the mind affected, are of importance to the medico-legal question, principally because lawyers have been in the habit of looking to one set of functions only as the subject of mental disease. We have on a former occasion fully expressed our opinions on the important question of moral insanity in the pages of this Journal (Nos. 24 and 25); and notwithstanding the metaphysical criteria of insanity which still maintain a place in the opinions of the least informed or most prejudiced of our legal authorities, we do not believe that proof of intellectual aberration will be deemed necessary to establish the existence of insanity in cases where an abnormal change in the affections and propensities can be demonstrated to have been the result of cerebral disease.

We cannot, however, omit all reference to a subject so important as that of delusion, a symptom which is still accepted in our courts as the most authentic mark of insanity, as the essence of cerebro-mental disease, if not as the disease itself. A clear appreciation of the nature of insane delusion is essential to the medical witness, since it is a topic on which barristers delight, by sophistical reasoning, to represent the opinions of medical men in an absurd and extravagant point of view, and in that manner to damage the value of their evidence.

What then, is, and what is not, a delusion? It cannot be doubted that

this term has been long undergoing a transformation in its meaning. This new sense both medical men and lawyers would do well to accept with a good grace, and to agree to use the term only in its scientific medico-legal signification. "Logicians cannot make the meaning of any but *scientific* terms; that of all other words is made by the collective human race." The word delusion has become a scientific term, and it is highly necessary for those who have to make use of it to agree upon what it shall be held to connote. No better example can be given of this necessity than the different, and indeed contrary, meanings which great legal authorities have given to the term. Thus, Sir John Nicholl gave it as his opinion that "a delusion is a belief in facts which no rational reason would have believed." If rational reason means the reason of a sound mind, this definition is an authoritative restriction of the term to its pathological sense.

But Lord Brougham defines a delusion as "a belief of things as realities which exist only in the imagination of the patient;" and thus completely reverses the meaning of the term as fixed by Sir J. Nicholl.

The objections to Lord Brougham's definition of delusion are that it includes the erroneous opinions entertained by some people, such as mesmeric prophecy, clairvoyance, and spirit rapping, and that it also excludes many of the actual delusions of the insane which are founded upon false perceptions of the senses. Such false perceptions, arising as they do from actual changes either in the nerves of sense or in the *sensorium commune*, cannot be said to exist only in the *imagination* of the patient. The phantasmagoria of Nicolai were owing to material changes somewhere in the optic tract; had he believed in their reality he would undoubtedly have been the subject of delusion, but of delusion independent of the imagination.

The nature of delusion has hitherto been greatly misunderstood, and has been the source of abundant mystification in medico-legal trials. A clear recognition of its nature would be of immense importance, and the hope of arriving somewhat nearer thereto will, we trust, justify the following summary of our own opinions on the subject:

"A delusion is a belief in the existence of things which have no existence in reality, or an erroneous perception of the nature of things, or of their relation to each other, occasioned by cerebro-mental disease. It may be said that this also is begging the question, and leaves the difficulty as great as before. This is to some extent true, but unavoidable. For it must be admitted that there is no certain method of distinguishing between the erroneous intellectual operations of a diseased mind, and those of the sane but imperfect reasoners who abound in society. Insanity is a disease recognisable with sufficient certainty by many symptoms when grouped together, not one of which, however, can safely be trusted to by itself, or considered pathognomonic. Most of the symptoms are occasionally to be observed singly where insanity does not exist. In illustration from another disease: pain in the breast is a symptom of pneumonia, which, although it does not always occur in that affection, and does occur in some others, is not without value when taken in connexion with other symptoms and signs. In the same manner, an absurd opinion, which taken by itself would possess little value as an indication of insanity, when considered in a group with other symptoms may become of great diagnostic value. Thus, the belief of the unfortunate parricide, Mr. Dadd, in Osiris and the religion of ancient Egypt, was far more dignified, and scarcely more absurd, than the religion of the Mormons, the Lampeter Brethren, the followers of Johanna Southcott or Swedenborg. It could not, therefore, on its own merits.

or demerits, be pronounced to be an insane delusion; but when taken in connexion with sudden change of habits and disposition, loss of rest, and other indications of nervous disturbance, and followed by the homicide of a beloved parent, that strange opinion is at once recognised as the fantasy of a diseased brain.

"Not unfrequently, but by no means constantly, the delusions of the insane possess characteristic features, by which they may be distinguished from the absurd opinions of the foolish and ignorant. The following are some of the most prominent :

"1. The delusions of the insane are generally independent of the opinions of others; they isolate the person who entertains them from his kind, whereas the sane portion of mankind are gregarious in their absurdities; fools are to be considered sane, follow each other through a gap like a flock of sheep, oftentimes indeed, following some bell-wether who is more rogue than fool: they have neither the confidence nor the courage to walk alone. Mr. Dadd was probably the only person in England who believed in Osiris; had there been a few hundreds, or even a few scores of persons entertaining the same belief, his ideas on this subject would have been of infinitely less value as a symptom of insanity.

"2. The faith of the insane in the truth of their delusive opinions is steadfast and unflinching. It almost surpasses the religious faith of the Mohammedan or Hindoo, and renders pale by contrast the attenuated belief which sane men accord to the absurdities of the hour.

"The *dilettanti* philosopher, or religionist, concedes to clairvoyance, to rapping spirits, or to Mr. Prince, a certain amount of belief, which may give way under the assault of ridicule, or logic, or misfortune; but the lunatic believes in his delusion with all his soul; he may outlive it, or be cured of it, but he can never be driven from it by any influences, however potent; 'no one who has not been insane,' says a convalescent patient, 'can imagine how terribly real the delusions of lunatics are.'

"3. The delusions of the insane come on after some physical or moral shock, and often present strange contrasts to the previous habits of thought, or have no relation thereto. The absurdities of the foolish or the ignorant have no such starting point, and are generally consistent with their customary attempts at thinking.

"4. The delusions of the insane, in many instances, have relation to the patients alone, and are often of a kind which renders their nature apparent. The idea of loss of personal identity, in an infinite variety of ways, is a frequent source of delusion, and indicates so complete an overthrow of the normal action of the mind, that it must generally be considered the result of disease. Even where such delusion is to a certain extent endemic, and therefore loses its isolated character, as in the *loup garous* of France, it is found to be caused by disease. In doubtful and difficult cases the psychopathist can only form his opinion by a careful estimate of all concomitant circumstances. The true test of delusion as a symptom of insanity is its origin or mode of production. Its existence corroborates the testimony of various physical and rational symptoms of cerebro-mental disease, and its own nature is discovered by their existence: they mutually prove each other."*

The little work from which we have quoted is one among others which has been produced by the encouragement given to the study of these and similar questions, by a nobleman whose labours in advancing the scientific knowledge of insanity and improving the condition of the insane have been so great and so successful, that we shall be readily excused for concluding this article by a brief reference to them.

It is not common to find among lawyers of high rank a just appreciation of the medical profession, especially of that section of it which

* *Unsoundness of Mind in relation to Criminal Acts*, by J. C. Buckhill, M.D., &c., 1854. (Sugden Prize Essay).

makes insanity its peculiar study. But many acts of enlightened liberality, directed to the advancement of psycho-pathological science, prove that the ablest and most profound lawyer of the age has never participated in those narrow prejudices which have too often found expression in sarcasm and rebuke, directed from the bench against medical witnesses, whose motives were at least honourable, if their opinions have not unfrequently been more unprecise and speculative than the purely practical and perhaps somewhat rude intellect of our judges could tolerate. It may be fairly presumed that Lord St. Leonards was by no means satisfied with the amount of knowledge respecting insanity which he found among medical men, but, instead of indulging in sarcasm and rebuke, he set himself manfully to work to remedy the defect. He not only established the Sugden prizes to be awarded by the Irish College of Physicians to the best essays on insanity subjects, but he urged upon Government the appointment of medical men to the superintendence of the public asylums in Ireland, and used the most strenuous efforts to establish a regular system of clinical instruction in those asylums. A complete account of his labours in the cause of the insane in Ireland would for ever associate his name with those of the greatest benefactors of this most pitiable section of the human race; with that of Pinel in France, and of Conolly in this country. When he first went to Ireland, he found both the law and the practice of lunacy in a deplorable condition. There was actually no law whatever for the regulation of private asylums, and the natural and inevitable consequence was, that the unhappy inmates were treated with great barbarity and neglect. Mechanical restraint of the worst and heaviest kind was constantly and habitually employed. The dietary was bad, the clothing worse; the attendance, after the old recipe of brute force, combined with stolid neglect.

To remedy this sad state of affairs, Sir Edward Sugden introduced a bill framed upon the model of the English statute, but with additions and improvements suggested by his own large experience. This bill became law, in spite of the strenuous opposition of the owners of asylums, who wrote pamphlets against it, and used every effort to obtain its rejection. At first this law was carried into effect by the inspectors of prisons, but ultimately separate inspectors of asylums were appointed, and the constant and enlightened supervision which they have exercised has been productive of the utmost benefit in ameliorating the condition of the insane in Ireland. But Sir E. Sugden took a more active and personal part than that of a legislator in effecting these reforms. He frequently visited the asylums within reach of Dublin, examined the patients, and inspected the wards, the bedding, the food, the rules, the books, &c. As it was never known when these visits of the Chancellor were likely to occur, the keepers of all asylums within his reach were kept on the alert, and a manifest improvement immediately took place. He interposed actively, and successfully, to introduce the non-restraint system of treatment, and some of the most painful instances of barbarous restraint were discovered and remedied by his personal intercession. When it was found, that the owners of the asylums in which the patients were in a state of wretchedness, could not be compelled by the law to remedy the consequences of their neglect, with a speed sufficient to satisfy

his ardent and earnest philanthropy, he himself supplied food, clothing, and skilful attendance, to the pitiable objects of his solicitude, and paid for them out of his own purse.

When Sir Edward instituted inquiries respecting the lunatics under the charge of the Court over which he presided as Lord Chancellor of Ireland, he found that no one knew who they were, or where they were, or in what condition they were. By advertisements, orders, and other means, he at length was enabled to make a correct list of them: he then compelled the committee of every lunatic to make a report to him on a fixed day every year, accompanied by a certificate from a medical man not connected with any asylum in which the lunatic was placed, as to his mental and bodily condition. By these means he in a short time obtained an accurate account of all lunatics under the charge of the Court of Chancery, with full particulars respecting their property and their personal condition. Thus it happened that when an application was made to the Court respecting any lunatic, the Chancellor had only to refer to his book to possess himself of more ample and accurate information than either the counsel or the solicitor could usually obtain. In this manner he was able to correct many abuses arising from short allowances, improper treatment of the person, and unfair dealings with the property. As an example of the latter, we may refer to an instance in which the next brother had taken possession of considerable landed estates belonging to the lunatic, had built a house upon them, and had in his marriage deed settled them as the owner, whilst from year to year he reduced his brother's scanty allowance, and kept him at an inferior farm-house. When Sir E. Sugden left Ireland, all these abuses had been corrected; and the information he had collected and systematized, and the rules he had laid down, were such as effectually to prevent their recurrence.

When, as Lord St. Leonards, he became Chancellor of England, this great and wise man undertook the complete revision of the statutes relating to lunacy in this country, and it is well known that he bestowed great care, attention, and labour upon this work. The result has been the production of an excellent and comprehensive code of lunacy laws. In matters having such wide relations, it is not surprising that defects should still exist; but it may safely be affirmed that, viewed as a whole, the lunacy laws of England are not only more perfect than those of any other country, but that they are also greatly superior to the statute laws of this country on almost all other subjects. The great evils which still exist are attributable not so much to defects in the law, as to the inadequate ability of those to whom its administration is entrusted. These statutes were framed and carried by Lord St. Leonards, who had learned by personal observation, to what grievances and abuses the insane of different classes of society were most liable to be subjected, and in what direction therefore the power of the law was most needed for their protection—who had shown himself to be influenced by the warmest feelings of humanity and compassion towards the objects of this legislation—and whose unrivalled powers of legal insight enabled him to construct those wise and comprehensive statutes for which all persons interested in the welfare of the insane, owe to this nobleman a debt of deep and lasting gratitude.

John Charles Bucknill.

REVIEW VIII.

1. *The Principles and Practice of Ophthalmic Medicine and Surgery.* By T. WHARTON JONES, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, London; Ophthalmic Surgeon to the Hospital, &c. Second Edition.—London, 1855. 12mo, pp. 549.
2. *A Guide to the Practical Study of Diseases of the Eye; with an Outline of their Medical and Operative Treatment.* By JAMES DIXON, Surgeon to the Royal London Ophthalmic Hospital, Moorfields; formerly Assistant-Surgeon to St. Thomas's Hospital.—London, 1855. Post 8vo, pp. 395.

THE appearance of a second edition of Mr. Wharton Jones's manual confirms, in the way most satisfactory to all parties, the favourable opinion given of the work in the 'British and Foreign Medical Review' for January, 1847. The result of much research, the manual is written with great care; while the materials, drawn necessarily from many sources, have been so thoroughly moulded in the author's own mind, that they are presented to the reader with much of the freshness of an original work.

Mr. Dixon's 'Guide' is neither so elaborate nor so systematic a work as that of Mr. Wharton Jones; but, as the title denotes, is intended chiefly to direct the attention of the student to the changes which the tissues of the eye present under various morbid conditions. In pursuing this object, the author has interspersed many original statements, evidently derived from his own experience, and worthy, therefore, of particular attention.

Both Mr. Wharton Jones and Mr. Dixon commence with an account of the methods of examining the eye, in order to a diagnosis, the only sure basis of successful treatment. The signs of eye-diseases, both *objective* or *anatomical*, and *subjective* or *physiological*, are described with some minuteness; and under the former head we find, in both authors, an interesting account of the methods of exploring the interior of the eye.

"The light," says Mr. Wharton Jones, "concentrated by means of a convex lens, about two inches in diameter, and three or four inches focus, is sufficient for all practical purposes, in the case of the anterior segment of the eyeball, comprising the cornea, aqueous humour, iris, pupil, and crystalline body." (p. 14.)

He then proceeds to give a succinct account, illustrated by diagrams, of the different forms of the new instrument, called an *ophthalmoscope*, for throwing light by reflection on the bottom of the eye. In a coloured plate he also shows the appearance presented by the retina in a congested state, with its transparency somewhat impaired, as seen by the aid of the ophthalmoscope, in a case of incomplete amaurosis. The following extract contains his estimate of the utility and application of the instrument:

"For all practical purposes, opacity of the lens can be sufficiently well ascertained by ordinary exploration with the pupil dilated. By means of the ophthalmoscope, adjusted for the purpose, however, otherwise undistinguishable opaque points can be seen. But it is of no advantage to push the diagnosis of opacity of the lens to such a minute degree of accuracy, seeing that we often meet with

cataractous opacity, quite evident to ordinary examination, notwithstanding which there is still pretty good sight; whilst, on the other hand, cases occur in which the impairment of sight complained of is not at all to be accounted for by the appreciable opacity of the lens.

"Whilst the morbid states of the anterior segment of the eyeball are sufficiently accessible to objective exploration, those of the posterior segment, comprising the vitreous body, retina, and choroid, could formerly, with some exceptions—(e.g., exuded matter in the vitreous body, serofulous and encephaloid growths at the bottom of the eye, &c., which give rise to a yellow, shining appearance, sometimes traversed by bloodvessels)—be determined only from the attendant subjective phenomena. This defect in our means of diagnosis of the state of the posterior segment of the eyeball was, however, practically little felt. Having determined that the disease was not seated in the anterior segment, and thus, *per exclusionem*, and from the nature of the subjective symptoms (together with the objective symptoms presented by the anterior segment, and by the eye considered as a whole), referred it to some part of the posterior segment, we were in a position to conduct our treatment of the case, not with less efficacy at least than can be done now, when it is possible, in many instances, to discover, by means of the ophthalmoscope, opaque spots, shreds, &c., in the vitreous humour, and congestion, with extravasations, exudations, and pigment deposits in or behind the retina.

"Morbid states discoverable in the vitreous body, are opacities in the form of fixed spots, or of undulating membranous shreds and filaments, of the most different size and form, with sharp or indistinct outline.

"There is nothing more easy than to see the vessels of the retina in a cat's eye without an ophthalmoscope. Having previously dilated the pupil by atropia solution, drop some water into the eye while the eyelids are held apart, and cover the cornea with a thin plate of glass. The vessels of the retina can then be seen, slightly magnified. It has been proposed to explore the bottom of the human eye in a similar manner, and instruments have been contrived for the purpose, but the ophthalmoscope is of more ready and convenient application.

"In man, the red colour which the bottom of the eye presents varies in tint; being brighter in fair, more of a yellowish-brown in dark, individuals. The retinal vessels are seen branching on the uniform red field formed by the more vascular choroid. At the entrance of the optic nerve, which appears whitish-yellow and well defined, the retinal vessels are seen emerging. The retina in the situation of the yellow spot is little or not at all vascular, and sometimes presents a greenish-grey aspect. A streak of pigment deposit may be seen at some part or all round the border of the optic papilla.

"The principal morbid appearances in the retina which have been observed, are congestions, spots of extravasated blood, pigment deposits, opacities of various aspect, the retina itself bulged forward by fluid accumulated between it and the choroid, and tremulous in the dissolved vitreous body." (Jones, pp. 28—30.)

In Mr. Dixon's 'Guide' the use of the ophthalmoscope is referred to on many occasions. The reader is probably aware that the form of instrument now generally employed is that of a single concave mirror, of four inches focus, perforated in the centre, and fixed in a handle; the mirror being used either alone, or with the addition of a convex or concave lens, according to the kind of illumination required, or the visual focus of the observer. This sort of ophthalmoscope goes by the name of 'Anagnostakis', after a young Greek physician. "I am informed, however," says Mr. Dixon, "that an instrument precisely similar to that introduced by Anagnostakis as his own invention, had been used by Graefe, of Berlin, in 1853, and shown by him to Anagnostakis during the same year."

The following extracts contain some directions and cautions respecting the use of the ophthalmoscope, so clear and important, that we cannot refrain from presenting them to our readers:

“The examination is made in a darkened room in the following manner:

“The patient sits sideways at the edge of a table on which a lamp is placed, the flame being close to, and on a level with, the suspected eye, but far enough back to prevent any light falling directly on the cornea: or he may sit with his back to the table, the lamp being close behind him, so that the rays of light just clear the top of his head, and fall perpendicularly, instead of obliquely, on the surface of the mirror.

“A chimney faintly tinged with blue, by decomposing the red rays, whitens the light, and imparts to the tissues of the eye a more natural appearance.

“Unless the pupil have become dilated by disease, it must be fully brought under the influence of atropine before the examination is begun.

“The surgeon, sitting close to and facing the patient, holds the mirror at such a distance in front of the eye to be examined, that the rays of the lamp may be brought to a focus on the patient's retina. As this cannot instantly be found by a beginner, it is better for him to direct the rays at first upon the patient's lower lid, and as soon as a well-defined inverted image of the flame has been formed there, a slight upward and forward movement of the mirror will bring the retina itself within the exact focus of the instrument.

“If it is desired to magnify the parts observed on the surface of the retina, or to increase their illumination, the surgeon, holding the ophthalmoscope steady in one hand, must take in the other a convex lens of about two and a half inches focus, and place it at a suitable distance in front of the patient's cornea.

“So much has lately been written about the value of the ophthalmoscope as a means of detecting incipient disease of the retina, that the student must be warned against the irreparable mischief he may inflict upon an eye, in which vision is only slightly impaired, by subjecting it to an intense glare of concentrated light.

“His first trials should be made on one of the lower animals,—a kitten, for example; and when he has acquired readiness in using the instrument, he may next proceed to examine patients who have long been hopelessly blind, but in whom the media of the eye remain transparent.

“One very important fact should never be lost sight of by those who employ the ophthalmoscope—namely, that the mere concentration of powerful light on the retina, if continued for more than a few seconds, does of itself place the part in an unnatural condition. In exploring the internal ear, by means of artificial light, we may, indeed, concentrate the rays upon the tympanic cavity, or its membrane, to any amount, without injury to the parts illuminated; but the retina, so far from being a merely passive object of examination, is just the one tissue in the body which appreciates the intensity of the rays which fall upon it; and it must be borne in mind, that an eye may be irritable and intolerant of light to an extreme degree, even although there may be a considerable diminution in its power of perceiving objects.

“Incipient amaurosis is a term in itself so vague, and often so indiscriminately employed to designate slight failure of sight from assumed loss of function in the retina, that I would most earnestly impress upon the student the danger of too hastily subjecting persons complaining of slight defects of vision to the searching glare of the ophthalmoscope. First, let the cornea be most rigidly examined, to ascertain whether some loss of transparency may not exist in the axis of vision. Then, after the pupil has been dilated, let the lens undergo a similar scrutiny; and until the observer has thoroughly satisfied himself that both these structures are transparent, let him not subject the retina to the possibly irritating effects of concentrated light.

“The chief value of the ophthalmoscope seems to consist in enabling the sur-

geon to set aside, as positively hopeless, a large number of cases formerly termed *amaurotic*, or *nervous*, which were assumed to be still curable, because their real nature could not be demonstrated.

"We now know that total disintegration of the vitreous body, detachment of the retina from its connexion with the choroid, and other equally hopeless conditions of structures essential to vision, may exist, without any alterations being produced in the outward appearance of the eye. In enabling us, therefore, to appreciate these conditions, the ophthalmoscope has proved of immense value; but the more delicate changes occurring in the early stages of deep-seated disease, whilst they elude detection by this instrument, are almost certain to be aggravated by its application." (Dixon, pp. 7--10.)

"We frequently meet with patients who have gradually lost sight in one eye or in both, and yet have neither suffered pain nor observed any unnatural appearances—such as spots, sparks, or flashes—on their field of vision. There is nothing faulty in the structure or functions of the iris; in fact, the eye presents every appearance of perfect health; and yet even the brightest light fails to make any impression on the retina.

"Such cases have been commonly classed as *amaurosis*; the absence of inflammatory action in the superficial tissues of the globe making the surgeon discard all thought of the choroid being the seat of the mischief. Yet the ophthalmoscope may at once reveal extensive changes both in the choroid and retina. These two structures may be widely separated from each other by effused fluid. The retina may be partially or wholly overspread with opaque, whitish deposit, or extravasated blood. The papilla of the optic nerve may be atrophied, so as to be barely traceable; the branches of the central artery or vein being fewer in number and diminished in size.

"In short, the whole fundus of the vitreous chamber may afford evidences of extensive disorganization, while the iris and superficial textures appear perfectly healthy. . . .

"It must needs be a difficult task to suggest any rational plan for treating a disease which, in many instances, begins by inducing mere dimness of sight, without any other noticeable symptom, either local or general. Chronic inflammation of the choroid, with effusion of fluid between it and the retina, sudden extravasation of blood in the same situation, and other changes, equally serious in their nature, are, as I have just stated, revealed by the ophthalmoscope in patients who have not been aware of any impairment of health, and who have suffered little, if any, local pain in the affected eye. In such cases the surgeon is commonly consulted after all the mischief is done, and when treatment can be of no avail. He will at least abstain from attempting violent measures, such as extensive bleeding and the action of mercury, which, while they are powerless to renew a disorganized structure, may seriously impair the general health of the patient." (pp. 174—6.)

"The invention of the ophthalmoscope has been hailed as the beginning of a new era in the study of eye-diseases, and as a means of clearing up all the obscurity which has hitherto surrounded the pathology of the choroid and retina; and doubtless the discovery of extensive organic changes in eyes which had never exhibited any of the ordinary signs of inflammation, has shaken old-established opinions as to the frequency of *functional amaurosis*; that is to say, loss of visual power in the retina without traceable change in its structure.

"Nevertheless, there are many limits to the powers of the ophthalmoscope.

"1. It can afford us a clear view of the retina only when the lens is transparent; and we know how commonly this body becomes opaque where disease has for a long time existed in the deeper tissues.

"2. Unless the pupil be of good size, or dilatable by means of atropine, only a very small extent of the retina can be brought into view; and chronic iritis,—so frequent an accompaniment of changes in the choroid and retina—never exists without giving rise to such adhesions between the iris and capsule of the lens as must limit both the size and dilatability of the pupil.

"3. Although various alterations in the retina—loosening of its attachment to the choroid, thickening, extravasation of blood, &c.—are so readily seen by the aid of the instrument, the earlier stages of disease (which alone are curable) produce changes so slight and delicate as to elude observation.

"A marked increase of redness in the colour of the retina sometimes takes place after it has been exposed for only a short time to the rays of the lamp; and the observer, who is not aware of this, may very likely attribute to a settled morbid state of the eye what is in truth only the temporary result of his own examination.

"When the rays reflected from the mirror are first allowed to fall upon the fundus of the eye, a generally diffused, reddish glare is usually perceived; but as soon as the proper focus has been attained—which is known by ramifying blood-vessels coming clearly into view—this red colour changes into an orange-red, an orange-yellow, or, in some cases, even a buff tint. The surface of the retina is now exposed, and as this is not quite a transparent tissue, but only translucent, it appears like a delicate whitish film, overspreading the red network of closely-packed choroidal vessels. The latter are not recognisable as separate trunks, but they produce the effect of an indistinctly mottled layer behind the retina.

"If the patient turns the eye inwards, the papilla of the optic nerve comes into view—a circular patch of white, faintly tinged with pink. From the centre of the papilla emerge the central artery and vein of the retina; the more frequent arrangement being that an artery and vein pass upwards, and a similar pair downwards; both sets of vessels then dividing into several branches, which run towards the periphery of the retina" (pp. 179—181.)

"There are certain forms of blindness which, from their partial extent or transient duration, we cannot suppose to arise from any such alterations in the retina as could be appreciated by the ophthalmoscope; and a search with that instrument, in these cases, would not only be fruitless, but, unless prosecuted with great care, would almost certainly produce mischievous results." (p. 187.)

"One of the most important results of the ophthalmoscope will probably be a great restriction in the administration of mercury. Patients who, for many months, have lost the perception of objects—perhaps even of light itself—will no longer be encouraged to submit to a lengthened mercurial course by the vague assurance that, 'as their case is one of amaurosis, a full course of mercury may give them a chance.' A view of the fundus of an eye overspread with *old* coagula; of a retina detached from the choroid by effusion of serum, and undulating with each movement of the globe; of an atrophied optic papilla; of a vitreous humour filled with opaque filaments and corpuscles; these, and other palpable signs of disorganization, will force the most devoted believer in the omnipotence of mercury to dethrone his idol." (p. 189.)

"The ophthalmoscope now shows us that morbid changes in the vitreous humour are by no means so uncommon as had been supposed. It is, however, far from easy to determine the precise nature of those changes; for not only is it difficult to bring into the proper focus the variously-shaped bodies, which appear as flakes, rounded granules, filaments, &c., but this difficulty is greatly increased by the rapidity with which some of the bodies are whisked about in the vitreous humour by each involuntary movement of the patient's eye. They are probably, for the most part, coagula of effused blood, mixed in some cases with granules of pigment, or the products of inflammation. Some of the larger, more membranous flakes suggest the notion of their being portions of broken-up hyaloid tissue." (p. 192.)*

These extracts from the works of Mr. Wharton Jones and Mr. Dixon, while they may serve as specimens of the style of writing of their respective authors, are valuable as exhibiting the amount of benefit, as

* For further information regarding the employment of the ophthalmoscope, we may refer the reader to an article by Mr. Wharton Jones, in this Review (Oct. 1854, p. 459), and another by Dr. Bader and Mr. Bransby Roberts, conjointly (April, 1855, p. 501).

well as the difficulties attendant on the new mode of exploring the deep-seated textures of the eye, as met with by men well qualified for observation, and attached to institutions where eye-diseases are extensively treated.

Two slight inaccuracies occur in Mr. Dixon's directions for the use of the ophthalmoscope.

At p. 7, he says the patient "may sit with his back to the table, the lamp being close behind him, so that the rays of light just clear the top of his head, and fall perpendicularly, instead of obliquely, on the surface of the mirror." If the mirror were held perpendicularly to the rays, their focus, formed by its reflection, would not enter the patient's eye, but would return towards the luminous object. The mirror must always be placed more or less obliquely in reference to the lamp, if the focus is to fall on the patient's eye.

He says further, that "A chimney faintly tinged with blue, by decomposing the red rays, whitens the light." The blue glass does not decompose the red rays. In the light of a lamp, the red and yellow rays are in excess, so that objects seen by it appear differently from what they do in daylight, which contains a large proportion of blue rays. The blue glass chimney, by adding blue rays, will improve the light of the lamp, by making it approach to the white colour of daylight.

Having occupied so much of the space allotted to this article with the topic of ophthalmoscopic exploration, we must now satisfy ourselves with a rapid review of some of the most interesting parts of Mr. Dixon's 'Guide,' leaving Mr. Wharton Jones' 'Manual' as a work whose character for accuracy, minuteness, and condensation is fully established.

Mr. Dixon arranges the remaining subjects of his work under the following heads:—Conjunctiva; Abnormal States of Sub-conjunctival Arcular Tissue; Cornea; Sclerotic; Iris; Inflammation of Iris and Cornea together; Choroid and Retina; Retina; Vitreous Body; Lens and Capsule; Diseases which Involve all the Tissues of the Eyeball; Diseases of Uncertain Seat; Lachrymal Apparatus; Eyelids; Orbit; Operations for Cataract; Artificial Pupil; Operations for Staphyloma, Strabismus, &c. In an appendix, a number of interesting cases are related.

Pterygium.—Under this head, Mr. Dixon having stated that it is the encroachment on the cornea that usually first alarms the patient, who applies to the surgeon under the apprehension of "a skin growing over the sight," adds, "there is, however, no real danger of this taking place, for, according to my own experience, the apex never extends so far over the cornea as to obstruct the area of the pupil." (p. 13.)

Purulent Ophthalmia.—In purulent ophthalmia, Mr. Dixon reprobates the depletory plan of treatment:

"If the treatment," says he, "of purulent ophthalmia by excessive depletion be judged by its results—the only sure test—we shall, I think, be forced to confess that there was ample cause for trying some less violent means of cure. It has been suggested that the more temperate habits of the mass of the people at the present day, as compared with what existed fifty years ago, may exert a considerable influence over the inflammatory manifestations of certain diseases, and that those surgeons who describe purulent ophthalmia as they saw it at the commencement of the present century, had really sometimes to contend with a greater fulness and force of circulation in their patients than we are in the habit of witnessing, especially among the overworked and crowded population of our great towns.

Certain it is that, as far as my own experience at a large metropolitan hospital enables me to form an opinion as to the general condition of patients suffering under purulent ophthalmia, I should say that they are uniformly more or less depressed, with a pulse more feeble than natural, and in a state which in every way contradicts general bleeding, and calls for the administration of tonics. There is usually a coated tongue, with loss of appetite, and a brisk purgative is needed at the very outset of the treatment. Afterwards, either bark and ammonia, or quinine, should be given, and hyoseyamus if the patient be restless. Pure air—to many the best of all tonics—must, if possible, be obtained; and all unnecessary confinement to bed, or to one room, avoided. Meat may be allowed once a day, and a moderate quantity of beer or wine; but on this head no arbitrary rule can be laid down. The surgeon's judgment must guide him as to the cases in which he ought to forbid stimulants, recommend them in moderation, or even insist upon an extra quantity being taken.

“Meantime, the local treatment should be commenced at once. I always employ either a solution of alum (eight or ten grains to the ounce of distilled water), to be injected under the lids every quarter of an hour, or nitrate of silver (three or four grains to the ounce), to be applied three times a day. It is useful, after employing the nitrate of silver for a few days, to change it for the alum, or *vice versa*. The application of the solid nitrate of silver to the whole surface of the inflamed conjunctiva is preferred by some surgeons, but I have not found it superior to the solution above mentioned; it may be used to the cornea in those cases (chiefly occurring, however, in *gonorrhœal ophthalmia*) where rapid ulceration is beginning at its margin.

“The student ought constantly to bear in mind that, although the disease termed purulent ophthalmia has received its name from that symptom which most readily attracts notice—namely, the profuse conjunctival discharge, the real source of danger lies in the *cornea*; and that, even if it were possible so to drain the patient of blood as materially to lessen, or even wholly arrest, the discharge, we might still fail to save the eye. It is not the flow of pus or mucus, however abundant, that should make us anxious, but the uncertainty as to whether the vitality of the cornea be sufficient to resist the changes which threaten its transparency.

“These changes are twofold—*rapid ulceration* and *sloughing*. Now, has any sound surgeon, I would ask, ever recommended excessive general bleeding and salivation as a means of averting these morbid changes from any other part of the body except the eye? And, if not, why are all the principles which guide our treatment of disease in other organs to be thrown aside as soon as it attacks the organ of vision?

“Do what we may, it must sometimes happen that, in the more acute cases of purulent ophthalmia, our best endeavours are in vain, and the cornea becomes irreparably damaged; still I feel convinced that, if we are unremittingly watchful to observe the changes which take place in the eye itself or in the general health of the patient, and to modify our treatment accordingly, a stimulating plan such as I have sketched will do all that our present knowledge of the disease can enable us to accomplish.” (pp. 32—34.)

Granular Conjunctiva.—The following is the advice of Mr. Dixon in one of the most desperate maladies of the eye—granular conjunctiva:

“I believe that in most cases of granular lid our chief dependence must be placed in improving the patient's general health, by giving him iron and quinine, singly or in combination, regulating his diet, and, if possible, placing him in a pure and bracing air. An issue in the skin of the temple, kept open with a single pea, and occasionally stimulated if the discharge becomes scanty, with some caustic or other irritant, is a slow, but often very serviceable adjunct. Tincture of iodine painted on the skin of the lids is also useful.

“I have at various times tried all the most approved lotions and drops, but have never satisfied myself that any of them were of much benefit. The acetate of lead in fine powder, dusted over the everted lid, produces considerable pain at the time of its application, but afterwards gives decided relief, apparently by mechanically

filling up the interstices of the "granulations," and so producing a smoother surface for the eyeball to move upon. As the salt slowly dissolves, it probably exerts also an astringent effect upon the vessels supplying the enlarged follicles and papillæ, and so diminishes the size of these excrescences." (p. 36.)

Gonorrhœal Ophthalmia.—"I do not remember," says Mr. Dixon, "to have seen a well-marked case of gonorrhœal ophthalmia in which both eyes were not affected, although it is certainly true that, very commonly, an interval of two or three days elapses before the second eye is attacked; and even then the disease is often much milder in one eye than in the other." (p. 38.)

Our own experience would lead to the conclusion, that gonorrhœal ophthalmia, a disease distinctly produced by the contact of the urethral discharge, seldom, if ever, affects both eyes. Such a course as is described by Mr. Dixon would lead us to suspect merely an accidental coincidence of non-specific purulent conjunctivitis with gonorrhœa.

Scrofulous Inflammations of the Eye.—In the treatment of this very numerous and distressing class of diseases, Mr. Dixon directs, that great attention be paid to the state of the general health. Thus, speaking of scrofulous conjunctivitis, at p. 49, he warns us, to—

"Take care not to confound the disease with mere conjunctival inflammation, or to suppose that medicines of specific action against scrofula (if any such there be) can afford a substitute for that general dietetic and other treatment which has for its aim the strengthening and soothing of an enfeebled and irritable system."

Scrofulous Iritis.—A case of scrofulous iritis, related at p. 157, confirms in a very striking manner the effect produced by proper attention to diet. Good beef-tea and other nourishing food were chiefly relied on, under which treatment the whole aspect of the eye was changed; effused blood and flocculent deposit were taken up from the pupil and the anterior chamber, and a solid nodulated mass, of a yellow colour, which had been thrown out on the surface of the iris, disappeared. The case is important, both as illustrative of the admirable effects of dietetical treatment, and confirmative of the occasional existence of a rare form of iritis.

Scrofulous Conjunctivitis.—The treatment recommended for this very troublesome affection is on the whole judicious, with the exception of a lotion of acetate of lead, which our author says may be employed. We had hoped this piece of barbarism had been wholly abandoned, from the fact being so perfectly established of the opaque and indelible deposits which form on ulcers of the cornea, when solution of acetate of lead is allowed to touch the eye.

A serious omission in the treatment is, that no notice is taken of the use, neither internal nor external, of belladonna, or atropine. Mention, no doubt, is made at page 52, of another medicine belonging to the class of mydriatics—namely, hyoscyamus, a pill of the extract being directed at night, or night and morning, when there is restlessness at night, or extreme intolerance of light during the day. The external medication from which by far the most remarkable effect in relieving the intolerance of light in scrofulous ophthalmia is obtained, is belladonna, or atropine, employed under the form of collyrium or drop; while small doses of belladonna leaf internally act also so beneficially, that we incline to place them next to quinine, and before the preparations of iron.

Conical Cornea.—The notion of some, that conical cornea is always preceded by a central ulcer, Mr. Dixon regards as disproved by the fact

which he has repeatedly verified, that in some corneæ, exhibiting the deformity in the most marked degree, not the slightest opacity has been found, either at the apex of the cone or elsewhere. That ulcer sometimes is a precursor of conical cornea, he confirms by the case of a child, aged four years, who was first brought to him with an opacity at the centre of the left cornea, the result of a small ulcer which had cicatrized. The cornea was very slightly conical, and, except at the centre, quite transparent. In twenty-two months after, the child was brought again, the sight of the eye having become very defective. The small cicatrix was in the same state as before, but the cornea had assumed a completely conical form. (p. 68.)

“As regards my own experience of conical cornea,” observes our author, “I have never found astringent or stimulating applications of the slightest use; nor have I felt warranted in performing any of the operations which have hitherto been recommended.” (p. 70.)

Abrasion of Epithelium of Cornea.—Mr. Dixon thus describes the effects of a slight scratch, scraping off a portion of the corneal epithelium:

“It would be impossible for any one who had not actually witnessed the effects of this seemingly trifling injury, to believe it could give rise to such acute suffering as it sometimes produces, especially in persons of an irritable nervous system. I have seen many almost fainting in consequence of the pain resulting from the edge of a sheet of paper, the cuff of a coat, or an infant’s finger-nail, coming in contact with the cornea, even although the abrasion of epithelium was so minute that it required the most careful examination to detect it.” (p. 94.)

Mr. Dixon states the most soothing application, and one which produces instantaneous relief, to be a drop of perfectly fresh castor or olive oil upon the surface of the cornea. Three days are generally required to effect a regeneration of the abraded surface, during which rest of the eyes should be observed. We have never thought of any application to the cornea itself, having always found the pain to yield speedily under the influence of extract of belladonna painted on the lids, and a pledget, wrung out of cold water, laid over the eye.

Corneitis, or Keratitis.—Those practised in the observation of eye diseases will readily acknowledge the accuracy and clearness of Mr. Dixon’s description of corneitis. With respect to the treatment, he expresses his “conviction that, in the vast majority of such cases, if not in all, both bleeding and mercurialization are most injurious.” (p. 75.) He also disapproves, and certainly with justice, of the local application of nitrate of silver. (p. 76.)

We are not disposed to coincide altogether in the rejection of mercury. To debilitate the patient by a full course of mercury, or to salivate, would certainly be wrong; but either by itself, or combined with quinine, mercury in alterative doses generally exercises so beneficial an effect in corneitis, that we must regard it as most valuable in a disease so apt to baffle the best directed treatment. Mr. Dixon says nothing of another excellent remedy in this very troublesome affection—namely, tartrate of antimony; and of the powers of belladonna administered internally he gives no hint.

Suppuration and Ulceration of the Cornea.—Mr. Dixon’s statement

(p. 79), that inflammation of the cornea, attended with the formation of pus among its fibres, or loss of its substance by ulceration, is invariably followed by a permanently opaque cicatrix, cannot be admitted. Such a result is frequent, perhaps general, but not invariable. Onyx, or abscess of the cornea, is often discussed, so as to leave no opacity; and a transparent cicatrix, at first depressed but ultimately on a level with the rest of the cornea, and leaving no trace of its existence, is not at all uncommon.

Neither can be admitted as consonant with fact, our author's account of the progress of an abscess of the cornea, when he says (p. 80), that pus continuing to be infiltrated more and more among the fibres of the cornea, "commonly the posterior lamina is the first to give way," so that "the pus slowly oozes into the anterior chamber, and sinks to the bottom of that cavity." On the contrary, we believe that, to use the language of Hunter, and adopt the truth of a principle which he established, there is a "susceptibility which the parts lying between an extraneous body and the skin have to ulcerate, while all the other sides of the abscess are not irritated to ulceration,"* a fact most remarkably illustrated by the uniform progress of pus through the anterior, and not through the posterior, laminae of the cornea. We believe that although in some very rare cases of abscess of the cornea a coincident exudation of lymph or of pus takes place into the anterior chamber, the bursting of the cornea is never into that cavity in the first instance, as Mr. Dixon describes it to be, but that the front of the cornea first gives way in every case, to be followed, often, but not always, by the laminae posterior to the abscess also giving way by ulceration, so that the whole thickness of the membrane is disorganized.

In speaking of the treatment (pp. 82—99) of suppuration of the cornea, Mr. Dixon omits all mention of one of the most effectual remedies—namely, evacuation of the aqueous humour. He notices the fact, that as soon as that fluid escapes through a rupture of the cornea, the pain subsides or ceases; yet overlooks the inference, that a small puncture will remove the tension of the inflamed textures, relieve the pain, and permit a healing action to set in—an inference, the truth of which was practically demonstrated by Wardrop.

Prolapsus Iridis.—Speaking of prolapsus of the iris through a perforating ulcer of the cornea, Mr. Dixon says (p. 85), that "in some books on ophthalmic surgery, the reader is directed carefully to push back again with a probe such protruded portions of iris." We dislike such vague phrases as "some books." If there is any book containing so absurd an advice, why not mention it expressly?

When prolapsus iridis occurs in incised wounds of the cornea, Mr. Dixon advises (p. 98) an attempt to be made to replace it with the small spatula. No doubt there is a difference between this case and that of prolapsus through an ulcer. In general, however, the attempt at reduction in the manner here advised will prove as fruitless in the one instance as in the other. The only method which has succeeded in our hands, for replacing a piece of iris prolapsed through an accidental wound of the cornea, has consisted in these three consecutive steps

* Hunter on the Blood, &c., part II. chap. vi. § 2c

first, bringing the iris under the influence of atropine; secondly, placing the patient in a state of insensibility by means of chloroform; and, thirdly, pressing the bit of iris back with a small probe.

Removal of Foreign Bodies from the Eye.—Under this head the reader will find a number of useful practical hints. It is remarkable, however, that nothing is said of the advantage of bringing the patient under the influence of chloroform, before attempting such operations as the extraction of splinters of metal buried deep in the cornea, lying in the anterior chamber, or fixed in the iris.

Iritis.—In all the varieties of this disease there is apt to occur a change in the colour of the iris. If blue, for instance, it becomes green. The change of colour is generally attributed to the presence of stagnant blood, or a deposit of lymph, in the substance of the inflamed membrane; but Mr. Dixon (pp. 127, 135) thinks it depends chiefly upon yellowness of the aqueous humour, or effusion of yellow lymph into the anterior chamber.

The mottled appearance of the internal surface of the cornea, which is often seen in syphilitic iritis, Mr. Dixon describes as a symptom chiefly of the rheumatic variety. He notices that, if of long standing, it is very difficult to remove. While yet recent, he recommends (p. 133) a blister to the temple, to be kept open with savine cerate.

Abscesses of the iris, such as so frequently occur in syphilitic iritis, Mr. Dixon describes (p. 136) as “masses of fibrin.” He notices that they are of the most varied colour, shape, and size: in some cases less than pins’ heads; in others, occupying a considerable portion of the anterior chamber; and yellow, reddish, or reddish-brown, according as they are recent, or of some duration, or in proportion to the number of vessels traversing them. The masses in question are generally described as *tubercles*; they are, in fact, abscesses, not mere masses of fibrin. Not unfrequently they burst, and give rise to small hypopya.

Mr. Dixon appears to be of opinion that the mydriatic power of belladonna is of no use in the treatment of the internal ophthalmia:

“Indispensable as atropine is,” says he, “in our examination of many morbid states of the eye, I do not regard it as of any service in iritis; for, as I stated at the commencement of this section, an inflamed iris loses its power of motion. Atropine, therefore, must be *useless* during the active stage of inflammation. At a later period, when the iris is beginning to recover its motory function, it may, I think, even *do harm*, and in the following way:—The hinder surface of the iris, termed ‘uvca,’ is covered with a layer of pigment-cells. When fibrin is poured out behind the iris (which no doubt happens in all cases of acute inflammation), these pigment-cells become, for a time, firmly united to the capsule of the lens; and if, when the iris is regaining its motory function, a forced dilatation of the pupil be effected by the influence of atropine, some of the pigment may be detached from the posterior surface of the iris, and left adhering to the capsule, forming those brown patches so familiar to us in patients who have suffered from iritis. Only get quit of the fibrin which is glueing the pigment-cells to the capsule of the lens, and the iris is at once effectually liberated.

“People sometimes talk and write as if occlusion of the pupil in iritis were the result of spasm of some sphincter muscle, the contractions of which could be paralyzed by atropine, and the pupil thus kept permanently dilated. But the real cause of closure is totally different from this. Fibrin is poured out from the surface of the iris and edge of the pupil, upon the front part of the capsule of the lens, overspreading the latter where it corresponds to the area of the pupil.

Now, if this effusion is not quickly removed by absorption, it becomes organized, and forms a membrane stretching across and blocking up the pupillary opening. Gradually this membrane contracts, and, in doing so, draws together the edges of the pupil, until that aperture is reduced, in some cases, to the size of a pin-hole." (pp. 138, 139.)

It would certainly save a great deal of trouble, both to the practitioner and the patient, if the use of belladonna could be safely dispensed with in the treatment of iritis; but we believe this cannot be done, and that Mr. Dixon's reasoning on the subject is fallacious. On a due employment of these three means of cure—depletion, mercurialization, and artificial mydriasis—the successful treatment of iritis depends.

With regard to what is stated by Mr. Dixon, we would observe, in the *first* place, that an inflamed iris does not lose its power of motion from the very commencement of the attack. The pupil still continues for a time to close during sleep, and expand when the patient is awake. It also dilates, and remains widely dilated, by belladonna, in the early stage of the disease. Belladonna being applied, and the patient bled immediately, the pupil will often be seen to become considerably expanded before the arm is tied up.

In the *second* place, the tearing away of the uvea from the iris, in consequence of belladonna, is an exceedingly rare occurrence, and, as far as we have seen, never occurs where bleeding and the use of mercury are had recourse to with promptitude.

In the *third* place, the little brown patches which are seen in many cases projecting from the edge of the pupil, do not arise from forced dilatation of the pupil, but are most frequently met with in neglected cases, where no belladonna has been employed.

Mr. Dixon's abandonment of belladonna may be regarded as equally extravagant and unjust as the notion of the late Dr. James Hamilton, jun., who supposed calomel to be superfluous in the treatment of idiopathic iritis, and actually attributed the successful cure of the disease to the belladonna:

"Admitting in its full extent," says he, "the signal success of the late ingenious Mr. Saunders, and of his very intelligent editor Dr. Farre, in cases of inflammation of the iris, which the author most willingly does, he ventures, with much deference, to suggest, that calomel had no other influence in the cure than in subduing the syphilitic virus. He is inclined to attribute the success chiefly to the external application of the extract of belladonna, which was so happily employed by Mr. Saunders; for it cannot be doubted, that if the radiated fibres of the iris be kept in a constant state of contraction, so as to dilate the pupil to the utmost extent, the minute vessels of that delicate part must undergo mechanically such a change as shall probably prevent effusion from their extremities."*

On the subject of *Syphilitic Iritis in Infants*, which has been too much neglected by authors on eye-diseases, the reader will find some valuable observations in Mr. Dixon's work, illustrated by cases.

Aquo-capsulitis.—Mr. Dixon quarrels with this convenient name for a well-marked disease, and proposes to substitute the lumbering appellation of "Inflammation of the Iris and Cornea together." *Aquo-capsulitis* means inflammation of the free surface of the aqueous cell, and although the parietes of the cell are formed by a variety of structures, the facts

* Observations on the Use and Abuse of Mercurial Medicines, p. 217. Edinburgh, 1819.

that the free surface is continually moistened by one and the same fluid—that the cell itself is closed at every point, so that its secretion, if morbidly increased, is retained within it, and can be removed only by absorption, or by an artificial opening—and that when inflammation affects the cavity, adhesions are apt to form between portions of its parietes, in consequence of effusion of lymph—are sufficient to vindicate the comparison which is made of the aqueous cell to the other shut sacs, and to excuse the name *Aquo-capsulitis*.

Mr. Dixon's description of the disease is short, but differs from what is usually given, in the statement (p. 168), that in the more acute cases hypopyon commonly occurs. This event we have never witnessed.

Cataract.—Mr. Dixon correctly states (p. 197), that the ancients supposed a considerable empty space to exist between the iris and lens, and that a certain humour, dropping into this space, coagulated there into a membrane in front of the lens. He goes on to say:

“The latter body they supposed to be quite unaffected by the disease, and this opinion was so firmly rooted in their minds that, even after using a needle to depress a cataract, they believed they had only removed an opaque skin out of the axis of vision, and had left the lens untouched, and in all its natural transparency. One sees, therefore, how readily they could trace an analogy between a membranous screen descending in front of the lens, and a *portcullis* let down before the gate of a fortress. Now, the original meaning of ‘cataracta’ is *portcullis*.” (pp. 197, 198.)

It happens, however, in the first place, that the ancients never applied the term *cataracta* to the disease in question; and that, in the second place, *cataracta* did not originally signify a *portcullis*.

The word *καταπάκτης*, from *πάσσω*, *I dash down*, originally signified a steep place whence water rushed down; by and by it was applied to a bar or flood-gate, such as might, by partially shutting up a stream, cause an artificial dashing down of water; ultimately, the word came to be occasionally used for what might shut up a door-way—i.e., a *portcullis*; but it was never employed by the Greeks to signify a disease.

The Greeks styled the disease which we call cataract, *ὑπόχυμα ὑγροῦ*, which means *the flowing down of a humour*. This name the Arabians translated into their own language by words which the mediæval translators out of Arabic into Latin rendered literally by *aque descensus*; whence, by way of synonym, or Latino-barbarous quibble, arose *cataracta*, as applied to the disease in question.

The catoptrical test Mr. Dixon regards as of no real value in deciding on the presence or absence of incipient cataract.

“Any opacity in the lens,” says he, “sufficient to interfere with the reflection of light from its two surfaces, must be readily appreciable, on simple inspection, by an observer endowed with powers of adjustment of vision for most minute objects, without which no one can profitably study the diseases of the eye.” (p. 195.)

We cannot agree with our author in considering the catoptrical test as valueless in distinguishing cases of suspected cataract. Even the eyes of observers perfectly capable of distinguishing minute objects, may be deceived by apparent want of transparency in the lens, when recourse to the catoptrical test would instantly demonstrate the real state of matters. A

melancholy illustration of the truth of this occurred some time ago to us in a gentleman, who called upon us to inquire whether we should consider the cataract, which he said we would see in his right eye, ready for operation, the left lens having already been removed, but unsuccessfully; for that disease, in London. The left eye bore the marks of a hapless extraction. The right lens appeared to us free from opacity, but had been pronounced by the operator on the left eye to be affected with incipient cataract. To neither eye had the catoptrical test been applied. In the right eye, the three images were perfectly normal; the case was one of amaurosis. The patient had come to London from a distant colony, under the impression that the failure of his sight was owing to cataract; he had paid a large sum of money for the removal of a sound lens; he had suffered pain, confinement, and deterioration of health, in consequence of an unnecessary operation; he had lost the only proper season for being successfully treated for the amaurotic affection under which he laboured; and all this under circumstances the real nature of which would have been revealed in a few seconds, had a lighted candle been passed before his eyes by any one even moderately acquainted with the catoptrical test. We consider it just to mention, that Mr. Dixon was not the operator in this deplorable case.

The point in question—namely, the utility or inutility of the catoptrical test in doubtful cases, being a very important one, we shall make no apology for presenting the reader with the opinion upon it of a gentleman well qualified to judge—namely, Dr. Hays, of Philadelphia.

“In the diagnosis of cataract,” says he, “the catoptric examination of the eye affords the most unerring test.

“In the early stages of lenticular cataract, the brilliancy and distinctness of the inverted image are diminished; it has no longer a sharp and well-defined margin, but its outline appears shaded off. This image gradually fades with the increase of the opacity, and long before the cataract is mature, the inverted image is obliterated. The deep erect image is also indistinct in the advanced stage, the anterior surface of the capsule giving only a general reflection.

“In capsulo-lenticular cataract, the inverted image fades much earlier than in mere lenticular cataract; a very slight degree of opacity of the capsule sufficing to destroy its function of reflection.

“Among the numerous cases we have seen, in which we have derived great assistance in our diagnosis from the aid of the catoptric test, we shall relate two, which will serve to show its utility, and to justify the confidence we repose in it.

“In September, 1839, I was invited by my friend, Dr. G. W. Norris, to examine a mulatto man, named Peter, in the Pennsylvania Hospital, who was supposed to be affected with glaucoma. The pupils had been dilated by the application of belladonna. There was opacity in both eyes, which was denser in some parts than in others. This opacity seemed more deep-seated than is usual in cataract, and its colour was of a greenish grey. Vision was, however, quite as good, perhaps better, than might have been supposed from the degree of opacity.

“On holding a lighted candle before the eyes, the three images were visible. The anterior upright image was natural in all respects. The deep-seated upright and inverted images were dull, their margins indistinct, and of an unusual reddish tint. The inverted image in one eye disappeared as the candle was moved opposite to the more nebulous portion of the lens; and when the observer looked at the eye of the patient obliquely, the second inverted image seemed to have a double point, like the letter W. I did not hesitate, from these phenomena, to pronounce it to be a case of cataract.

“A few days afterwards this man died suddenly, and we were afforded an opportunity of examining his eyes.

“The transparency of both capsules was impaired. A narrow streak at the posterior part of one lens, extending from near the margin to the centre, was quite opaque, and on applying a needle to this portion we found it quite soft, so as to be readily removed, leaving a depression. On carefully washing both lenses, so as to remove their superficial layers, which were softened as well as partially opaque, the remaining portion of each was found perfectly transparent and of a beautiful amber colour. This colour was the same whether the lens was viewed by transmitted or reflected light.

“The second case was that of a man named Christian Minster, forty years of age, admitted into Wills' Hospital, October 7th, 1841. This man stated that he had recently come from the country; that he had been admitted in one of our public institutions, where he was pronounced to have cataract, and it was determined to operate upon him; but that, being unwilling to submit to this, he had eloped. In a letter which he showed from his physician in the country, his disease was said to be cataract.

“The opacity behind the pupil had certainly great resemblance to that of cataract. The degree of vision he enjoyed corresponded to the degree of opacity; the independent and associate motions of the iris were tolerably active, and the patient saw best by twilight.

“On applying the catoptric test, however, it was at once found to be a case of amaurosis, and not of cataract. The three images were visible, and of their natural appearance. The history of the case led me to ascribe the amaurotic affection to congestion of some portion of the nervous apparatus, and a course of treatment corresponding to this view was directed, consisting of counter-irritation to the back of the neck, purging, stimulating pediluvia, &c. Under this course he improved so rapidly, that in a week he could read with one eye a diamond-print Bible. He subsequently had a relapse; but by the application of cups to the head, pustulation with tartar-emetic on the back of the neck, and afterwards ptyalism, he was completely restored. He was discharged cured in January following.”*

If the catoptrical test be capable of assisting, even in a moderate degree, in assuring an accurate diagnosis between incipient cataract and incipient amaurosis, it is surely worthy of cultivation by practical men. If it affords the smallest chance of avoiding such deplorable results, as occurred in the case which we have related, or as were likely to happen in Dr. Hays' second case, in place of being scouted as “of no real value,” it deserves the utmost possible attention.

Hard Cataract.—With respect to the formation of hard cataract, Mr. Dixon has adopted views similar to those of M. Malgaigne, and believes the ordinary progress of the disease in elderly persons to be in *striae*, proceeding from the circumference of the lens towards its poles.

“From puberty to the age of forty,” says he, “opacity of the lens is rare, except as a result of injury. When it does occur spontaneously, it is *cortical* in its origin; the margin of the lens first exhibiting opaque striae, which gradually converge as they extend themselves along its anterior and posterior faces. Between forty and fifty, cataract may still be said to be rather a rare disease; but from fifty to sixty, and still more after the latter period, it is the defect we naturally expect to find when a patient consults us for failing sight.” (pp. 212, 213.)

Mr. Dixon had already explained that when the *nucleus* of the lens

• “Becomes cataractous in an old person, the change seems to consist in a process of drying and atrophy of its fibres, although its amber tint, and a certain degree of translucency, are still retained.” (p. 199.)

* Hays' Edition of Lawrence on the Diseases of the Eye, p. 675. Philadelphia, 1854.

He goes on to state, that it is impossible to fix the exact time of life at which the peculiar morbid changes, now referred to, take place in the nucleus :

"Perhaps," says he, "sixty might be named as the age after which it may be expected to occur; although I am perfectly convinced, from repeated and careful examination of patients, that even to extreme old age it is much more common to find marginal opacity beginning while the nucleus is still clear, than to find nuclear opacity beginning while the periphery of the lens remains transparent." (p. 213.)

"The following is the ordinary progress of cataract in elderly persons:—At first, opaque streaks appear at the extreme edge of the lens; most commonly I have observed the lower edge to be first affected; the streaks gradually coalesce into patches, and spread themselves over the posterior face, a few only extending a short distance along the anterior face. The cataract may perhaps remain in this state for a year or more; then the whole body of the lens becomes slightly hazy, but not so much so as to prevent the posterior radiated opacity from being recognised. Gradually the opaque streaks advance farther and farther along the anterior face of the lens, until they appear within the area of the undilated pupil. By this time the general haziness has increased so much that the posterior face of the lens is hidden from view, even when concentrated light is thrown into the eye; and the opacity—both the linear and the diffused—becomes denser and denser, until little more than the anterior surface of the lens can be seen. Cataract is then complete, and vision commonly restricted to mere perception of light.

"As years go on, the surface of the lens usually becomes more and more opaque and white, in consequence of the increased deposit of earthy and fatty matter. The latter sometimes assumes the form of cholesterin, in such large crystals as to give the surface of the cataract a sparkling appearance." (pp. 215, 216.)

While Mr. Dixon acknowledges the priority of M. Malgaigne in the doctrine, that the opacity of cataract commences in the superficial laminae, and generally towards the edge of the lens, he seems rather dogmatically wedded to it, and fond of representing the belief of preceding authors to be that the disease begins in the nucleus, and gradually works its way towards the surface. That the opacity commences in the very kernel, and gradually invades the lens from this point towards the surface, till the whole is opaque, does not appear to be the opinion of any ophthalmologist. When Mr. Middlemore says, "Lenticular cataract generally commences with a dulness of the centre of the lens;"* or when Mr. Morgan tells us that "Hard cataract usually begins to form in the centre of the lens;"† these authors probably mean by centre merely that portion of the lens which is seen through the undilated pupil, and where the opacity is, in general, first discerned; the opaque streaks which sometimes fringe the margin of the lens in the incipient stage of the disease not being visible unless the pupil is widely expanded by belladonna.

The fact is, as Mr. Dixon himself acknowledges, there is a twofold change observable in any fully developed senile cataract. The laminae next the surface are soft, and present a whitish opacity; while the internal half of the lens is firm, amber-coloured, and sufficiently turbid to prevent any object from being seen through it. The changes which produce these appearances do not in all cases go on, *pari passu*. The nuclear change in many cases advances before the superficial, and *vice versa*. The nuclear change has also attracted more the attention of some observers, and the superficial more than that of others. This explanation will go far, we think,

* Middlemore on Diseases of the Eye, vol. ii. p. 84. London, 1835.

† Morgan's Lectures on Diseases of the Eye, second edition, p. 175. London, 1848.

to reconcile the descriptions of Tyrrell, Wharton Jones, and others, with the statements of Mr. Dixon.

Fluid Cataract.—Under this name, Mr. Dixon describes what is oftener called Morgagnian cataract, or *cataracta fluido-lucida*.

“In leucos,” says he, “which have become opaque, either from disease or injury, the softening process will sometimes slowly go on until nearly the whole mass is converted into a fluid state.

“The most uniform characteristic of fluid cataract is a total absence of those radiating streaks so evident in ordinary cataract, where the fibres of the lens, although deprived of their transparency, still retain their natural arrangement.” (pp. 217, 218.)

He goes on to describe the colours and other appearances of the kind of cataract in question; but the most remarkable of them all has escaped his notice—namely, the change which takes place in the colour of the cataract according as the patient sits erect or lies on his back. In the former position, with the head gently bent forwards, the cataract presents a brownish colour, owing to the nucleus of the lens gravitating forward towards the pupil; but the instant the patient lies down on his back, the cataract assumes a white colour, from the nucleus falling back towards the vitreous humour.

Our limits prevent us from entering minutely on Mr. Dixon’s account of the operations performed on the eye for the cure of cataract, or for other diseases.

Spring Speculum.—He frequently recommends the employment of a spring speculum for holding the eyelids apart during operations on the eye. Even in children, about to be operated on for congenital cataract, he appears (p. 317) to use this contrivance; also, in the operation suggested by Mr. Bowman, of tearing open an opaque capsule with two needles, introduced, one in each hand, through the two sides of the cornea (p. 321). In operating for strabismus (p. 366), the spring speculum is again recommended; as well as in forming an artificial pupil (p. 356), an operation in which it is generally so important to prevent the premature escape of the aqueous humour, that we doubt much the propriety of dispensing with the manageable pressure of the fingers of an assistant for the action of a mechanical contrivance which cannot be controlled.

Maunoir’s Operation for Artificial Pupil.—Mr. Dixon has fallen into an error regarding this subject, when he says—

“Maunoir’s method of cutting out a piece of the iris with scissors, in addition to other difficulties, has the defect of requiring a large corneal wound to be made before the scissors can be introduced; and, however simple it may look in a diagram to snip out little triangular bits of the iris, any one who has tried to do so on the dead subject will have found that even the finest and sharpest scissors cannot divide, with precision, such a slabby and yielding tissue as the iris becomes the moment the aqueous humour has escaped.” (p. 355.)

Maunoir made no such attempt to cut out a piece of the iris as is here attributed to him. His operation consisted merely in dividing the iris by two incisions divaricating from each other, leaving the intervening membrane to shrink.

Chronic Dacryocystitis.—Mr. Dixon’s treatment of this very common and annoying complaint appears singularly inefficient. After speaking of the injection of various fluids; either through the puncta or up the nasal duct; of astringent solutions dropped into the corner of the eye, or used as lotions; and of various other means, as abandoned or worthless—

"The remedy," he says, "I have found most serviceable is a little oval blister, about half an inch long, placed directly over the sac. This may be repeated every fortnight or so, according to the state of the skin; and the plan, to be efficacious, must be persevered in for several months. Astringent drops may be used at the same time." (p. 268.)

"The chief objection to the style (apart from its unsightly appearance) is that it requires management and care such as very few patients indeed can, or will, bestow upon it. They neglect to remove it and cleanse it regularly, and when it sets up some irritation (as it occasionally will do) they probably remove it altogether, and are unable to replace it. These and other reasons have induced me almost to abandon its use, and I do not think I have introduced ten styles within as many years." (p. 269.)

Stricture of the nasal duct is generally at the bottom of chronic dacryocystitis. The question then comes to be, how is the stricture to be removed with least trouble to the patient? We have no hesitation in answering, by opening the sac and passing a probe, of about one-twentieth inch thick, down the nasal duct. The incision into the sac ought to be pretty free, so that the probe may enter easily into that cavity; the probe is then to be directed nearly perpendicularly downwards, and pressed firmly through the stricture. It is rarely the case that the freeing of the stricture in this way does not require a very considerable pressure; under this the stricture yields, and the end of the probe is felt to strike against the floor of the nostril. The probe may be kept in for a few minutes only, or for an hour or two, as is convenient. It sometimes happens that a single passage of the probe, in this way, effects a complete and permanent cure; and with patients coming from the country, and eager to return home, this plan may be tried. The wound requires no further attention. It is better, however, to repeat the passage of the probe daily, as long as the wound keeps sufficiently open to permit that to be done without making any new incision, which is generally the case for fourteen days, or longer. A series of probes, gradually increasing in thickness, may be employed; and during their use attention is to be directed to the general health of the patient, which is often deranged; and such local means employed as are suitable for reducing the inflamed state of the skin over the sac, and removing any catarrhal or tarsal ophthalmia which may be present. From long experience of this simple treatment, we can recommend it as greatly superior to the employment of the style, or to any other plan which has hitherto been proposed.

Entropion and Ectropion.—The operations for the cure of these affections, falling so much under the care of the ophthalmic surgeon, Mr. Dixon omits to consider, for a reason which strikes us as altogether unsatisfactory.

"The operations," says he, "of entropion and ectropion, involving as they do merely such tissues as are met with in other parts of the body, need not therefore here be specially described." (p. 369.)

Nævus.—Mr. Dixon (p. 286) recommends subcutaneous ligatures, or the introduction of needles coated with fused nitrate of silver, as preferable to the including of any considerable portions of skin within the ligature, or the extensive use of escharotics. He states that he had seen injections of alum cause sloughing of the lids and great subsequent deformity.

Epithelial Cancer, affecting the skin over the lachrymal sac, Mr. Dixon (p. 289) treats with chloride of zinc, applied as a paste over the entire

surface of the sore. This application very effectually destroys the growth, and may be repeated as often as any part of the border appears inclined to extend itself.

Strabismus.—Under this head our author takes occasion to mention that division of the external rectus is very rarely required, but may be resorted to for the purpose of remedying the unsightly abduction which sometimes occurs when the section of the adductor muscle has been accompanied with too extensive separation of its connexions.

“I lately saw,” he adds, “a patient whose appearance had been remarkably improved in this way by the skill of my colleague, Mr. Critchett. The man came to him with the left eye extremely abducted, and quite fixed; and, as the semilunar fold had also retracted, his expression was most disagreeable. Mr. Critchett first divided the tendon of the external rectus, and afterwards removed the cicatrix of the old operation, loosening the conjunctiva very thoroughly from the inner side of the sclerotic. He then brought together, with fine stitches, the gap he had made in the conjunctiva, so as in a certain degree to draw out again the plica semilunaris. The operation was tedious and difficult, but the object was perfectly attained. All deformity was removed; and when I saw the case, some months afterwards, there was no trace of the unsightly leer which the poor man had formerly exhibited.” (p. 368.)

Amaurosis.—There is probably no subject in ophthalmology more likely to receive additions and corrections, in proportion as it is investigated on sound pathological principles, than amaurosis. It is universally acknowledged that the methods hitherto pursued in treating this subject have been very insufficient for the purposes of the practitioner.

“I would wish,” says Mr. Dixon, “to impress strongly upon the student, that although, for convenience sake, we may still continue to use the word *amaurosis*, we do not thereby express any opinion whatever as to the real pathology of dim sight or blindness. Amaurosis implies no definite and ascertained disease; it is only a word expressive of our own ignorance as to the cause of our patient’s blindness.”

“The descriptions of the older ophthalmic writers have so popularized amaurosis and its symptoms, that most students, before commencing the practical study of eye diseases, form to themselves some ideal type of an amaurotic patient, with his ‘vacant stare,’ and ‘widely dilated pupils;’ and have commonly a vague belief in the exclusive power of *mercury* to effect a cure.

“Nothing can be more unfounded than such sweeping generalizations.” (pp. 182, 183.)

The following are judicious cautions:

“If a patient comes before us, complaining of dimness of sight, attended with the constant or occasional appearance in the field of vision of moving clouds, dark disks, spots, sparks—in short, of any of the manifold subjective symptoms of disordered retina, we are not hastily to call the case *amaurosis*, and forthwith proceed to treat it according to any fixed routine.

“Our first business should be to ascertain *for ourselves* what sight the patient really possesses; testing his power for near things with type of different sizes, and, for those more distant, by directing his attention to various objects about the room, or across the street. Some patients involuntarily exaggerate their defects of sight to such an extraordinary degree, that their own accounts are absolutely valueless.” (p. 184.)

A remark which our author makes (p. 344) respecting the occasional failure of the operation of extraction, leads us to reflect, that the same “degeneration of tissue in the bloodvessels supplying the globe, and a weak condition of the heart itself,” to which he attributes the loss of

the eye in that case, may also, in many instances, have to do with the production of amaurosis. It seems to be necessary, in investigating cases of amaurosis, to pay a much more extended and more minute attention to the state of the constitution and to the antecedent diseases of the patient, than is commonly done. It is rarely the case that amaurosis is a merely local affection, unconnected with general bodily disorder. The inquirer should have in his mind, if not in his hand, while examining an amaurotic patient, a catalogue of the chief affections, general and local, likely to produce or to complicate the disease—a catalogue somewhat like that lately given by Dr. Lauder Lindsay,* of the complications of insanity. Such a plan of proceeding might prevent the mistake so frequently fallen into, of singling out some one antecedent of amaurosis as the cause of the disease, while the rest are passed over as merely subordinate conditions. The catalogue might run somewhat as follows:

- I. Cachexies and General Systemic Affections:
 1. Plethora; 2. Anæmia; 3. Scorbutus; 4. Scrofula; 5. Syphilis; 6. Fevers.
- II. Diseases of the Skin and Cellular Tissue:
 1. Cutaneous Eruptions; 2. Carbuncles and Boils; 3. Erythema and Erysipelas; 4. Ulcers and Abscesses.
- III. Diseases of the Digestive System and Alimentary Canal:
 1. Dyspepsia; 2. Chronic Vomiting; 3. Constipation; 4. Diarrhœa; 5. Dysentery.
- IV. Diseases of the Respiratory System:
 1. Bronchitis; 2. Asthma; 3. Influenza.
- V. Diseases of the Circulating System:
 1. Heart Disease; 2. Aneurism; 3. Varicose Veins; 4. Hæmorrhoids.
- VI. Diseases of the Liver and Kidney:
 1. Hepatitis; 2. Diabetes; 3. Albuminuria; 4. Oxaluria; 5. Dropsy.
- VII. Diseases of the Brain and Nervous System:
 1. Fractures of Cranium; 2. Tumours of Dura Mater; 3. Morbid Formations in Brain; 4. Wounds of Fifth Nerve; 5. Epilepsy; 6. Paralysis; 7. Hysteria; 8. Mania.
- VIII. Diseases of the Genito-Urinary System:
 1. Gonorrhœa and Gleet; 2. Spermatorrhœa.
- IX. Diseases of the Uterine System:
 1. Menorrhagia; 2. Leucorrhœa; 3. Amenorrhœa.
- X. Blood Diseases, affecting the Bones and Joints:
 1. Rheumatism; 2. Gout.
- XI. Chronic Poisoning:
 1. By Alcohol; 2. By Tobacco; 3. By Lead; 4. By Mercury.
- XII. Diseases of the Organs of Special Sense:
 1. Ophthalmia; 2. Otorrhœa; 3. Deafness; 4. Anæsthesia; 5. Neuralgia.

* Journal of Psychological Medicine, p. 70. January, 1855.

In perusing Mr. Dixon's work, we had noted many other passages besides those which we have quoted in this analysis, sufficiently interesting for remark. We also purposed directing our readers' attention to several valuable details of cases—such as, one of prolonged sojourn of a foreign body in the anterior chamber (p. 162); one of accidental transfixment of the lens from side to side with a pin (p. 220); two of acute choroiditis (p. 231); one of melanosis within the vitreous chamber (p. 239); one of serofulous deposition at the bottom of the eye (p. 243), similar to a case given by Panizza; one of total detachment of the iris (p. 372), and several others; but as this article has already gone far beyond the limits we contemplated, we shall extend it no further than cordially to recommend the work to the study of all who feel interested in the treatment of eye-diseases, and who are anxious to know something of the experience of one who has had ample opportunities, and possesses excellent talents, for observation.

W. Mackenzie.

REVIEW .IX.

1. *The Identity or Non-identity, or the Specific Cause, of Typhoid, Typhus, and Relapsing Fevers.* By WILLIAM JENNER, M.D., Professor of Pathological Anatomy in University College, London. ('Medico-Chirurgical Transactions of London,' vol. xxxiii.) 1850.
2. *Clinical Reports on Continued Fever, based on Analyses of One Hundred and Sixty-four Cases; with Remarks on the Identity of Typhus and Typhoid Fevers, &c. &c.* By AUSTIN FLINT, M.D., Professor of Principles and Practice of Medicine and of Clinical Medicine in the University of Buffalo.—*Buffalo*, 1852. 8vo. pp. 390.
3. *The Gulstonian Lectures on the Acute Specific Diseases.* By WILLIAM JENNER, M.D., &c. &c.—*London*, 1853. 12mo. pp. 55.

THE subject of Fever, in all its varied aspects, has been at different times so thoroughly discussed in the pages of this Review, as to lead us at once to apprise our readers of the limited, though not on that account less interesting, nature of our present inquiry. We propose to examine the evidence which has up to this time been adduced in regard to the question of the separate and distinct nature of certain forms of continued fever—in other words, to inquire if the existing state of our knowledge permits the conclusion that typhus and the so-called typhoid fever are separate and distinct diseases, and if relapsing fever is likewise entitled to be so regarded. As respects fever, this question of the identity or non-identity of certain of its forms is emphatically the question of the day. For, while at the outset we are prepared to acknowledge that the facts which have been accumulated and brought forward during late years have been sufficient to enable us to make up our own mind as to the non-identity of the three diseases now named, we are also aware of the different notion which is still entertained by many whose opinions are justly entitled to the fullest consideration, and shall therefore feel it our duty in stating our own view, to do so with becoming deference. As the subject of the morphological changes occurring in fever, to which the recent most important

investigations of Virchow and Dr. Parkes have added a new interest, will fall to be discussed in another article, our present inquiry chiefly aims at the nosological classification of fevers by the contemplation of their origin, symptoms, and course.

That the distinction of certain forms of continued fever, and their recognition as separate diseases, should have remained to be achieved by the physicians of our own day, need create no surprise, when we reflect that for many years small-pox, measles, and scarlet fever, now universally acknowledged as distinct, were believed to be the same disease, and were confounded under one name. Such being the case, as little surprise need be occasioned by the fact of many enlightened physicians still regarding the three forms of fever already named as varieties of one disease.

The admirable description of typhoid fever contained in the illustrious work of Louis,* "by affording a standard of comparison," as has been well remarked by Dr. Jenner, "materially lightened the labour of separating from that disease those which had previously been grouped with it." Louis' work was originally published in 1831; previous to that time, several French physicians—among others Prost,† Petit,‡ and Serres, Bretonneau§ and Trousseau||—had written fully on this form of disease; while by Bretonneau in particular it had been accurately described under the name of *Dothinentérie*, or *Dothinentérite*. An English physician—by name W. Stark—is mentioned by M. Andral as having given, in 1788, a very fair account of the alteration of Peyer's glands. Stark subsequently died of the disease he had himself described.¶ The revolution of prevalent views in regard to fever was, however, mainly accomplished by Louis when he described his *fièvre typhoïde*. During the interval which elapsed before the publication of a second edition of his work, which, strange to say, did not appear (and then in a considerably enlarged form) till 1841, the distinguished author had not only the satisfaction of finding his observations amply confirmed by his own countrymen—Lombard (who studied the typhus fever of this country), Chomel, and others—but by many foreign physicians, particularly in America. Of the latter, M. Louis, in the preface to his second edition, cites the names of about a dozen; among others, those of Dr. Gerhard** and Professor Jackson,†† to whose contributions of sterling value we shall have occasion to refer in the course of this article. In our own country, the excellent papers of Mr. Kennedy, of Dublin, and Dr. A. P. Stewart,‡‡ now of the Middlesex Hospital, were the earliest to give a faithful description of the symptoms

* *Recherches Anatomiques, Pathologiques et Thérapeutiques sur la Maladie connue sous le nom de Fièvre Typhoïde, &c.* Par P. C. A. Louis. Deuxième édition. Paris, 1841.

† *Médecine éclairée par l'Observation et l'Ouverture des Corps.* Paris, 1804.

‡ *Traité de la Fièvre Entéro-Mésentérique.* Paris, 1818.

§ *Archives Générales de Médecine.* Première série, tom. xxi.

|| *De la Maladie à laquelle M. Bretonneau a donné le nom de Dothinentérie ou Dothinentérite*, par M. Trousseau: *Archives Générales de Médecine*, 1826.

¶ *Pathologie Interne*, vol. iii. p. 665; Stark's Works, 4to, London, 1788.

** *The American Journal of the Medical Sciences for 1834 and 1837.*

†† *A Report founded on the Cases of Typhoid Fever, or the Common Continued Fever of New England, which occurred in the Massachusetts General Hospital, &c. &c.* By James Jackson, M.D. Boston, 1838.

‡‡ *Some Considerations on the Nature and Pathology of Typhus and Typhoid Fevers, applied to the Solution of the Question of the Identity or Non-identity of the two Diseases.* By Alexander P. Stewart, M.D.: *Edinburgh Medical and Surgical Journal*, 1840.

and pathological appearances in cases of typhoid fever.* In the conclusion of his paper, Dr. Stewart puts the question—"Are typhus and typhoid fevers identical, or are they not?" While hesitating to give a direct reply, it is very evident how Dr. Stewart's opinion tends; while, after perusal of the whole article, and of Mr. Kennedy's, more especially in connexion with the papers of the American physicians already named, we have been impressed with the feeling of a strong probability, though not actual proof, of the two diseases being separate and distinct, having been made out. The actual demonstration, however, of the non-identity of typhus and typhoid fevers has remained for Dr. Jenner to accomplish, and we are very ready to acknowledge that by him the greatest amount of light has been thrown upon this difficult but most important and interesting subject.† This belief we have indeed very great pleasure in avowing, because we are aware of no other papers on any scientific medical question which in themselves have more completely evinced the great amount of trouble in research, and devotion, with which the whole inquiry has been conducted by their author.

Aided chiefly by Dr. Jenner's observations, and those of Dr. Flint, we now proceed to exhibit, in as succinct, but at the same time as clear, a manner as possible, the present state of our knowledge on the subject of the difference between typhoid fever and typhus. We shall commence with the consideration of the course, symptoms, and lesions of the two diseases, and conclude with the view which Dr. Jenner has offered, of the difference in their specific cause.

The origin of typhus is certainly more distinctly marked than that of typhoid fever, the occurrence of crises and critical days in the former are more easily recognised, and the duration of the disease is more strictly limited and altogether shorter than in typhoid. "The mean duration of typhus," writes Dr. Stewart, "is about one-half that of typhoid fever;" and the same author, after speaking of the occurrence in typhus of a perceptible crisis, adds,

"In reply to the question, whether this is the case in typhoid fever, I can only adduce my own experience when I state, that neither in the numerous cases I saw in Scotland, nor in those I watched in Paris (about a dozen of them very carefully), have I ever seen anything approaching, in the remotest degree, to what I have noticed so frequently in typhus."

* Peculiarities in the types of prevailing fever, and more particularly as respects the morbid changes visible in the course of the intestinal canal, had about the same time arrested the attention of various other observers in this country. See, for example, the interesting account given by Mr. Goodsir (now the distinguished professor of anatomy in the University of Edinburgh) to the late Dr. John Reid, of the fever prevalent at Anstruther, in Fifeshire. *Edinburgh Medical and Surgical Journal*, p. 459. 1839.

† Dr. Wood, of Philadelphia, in his admirable work on the 'Practice of Medicine' (vol. i. p. 360), says:—"It was in the results obtained by the careful post-mortem examinations made in the Philadelphia Hospital by that distinguished pathologist (Dr. Gerhard), in conjunction with Dr. Pennock, and their no less careful investigation of the symptoms during life, that we first obtained positive proof of an essential distinction between the two diseases, which Louis himself had previously been disposed to consider as identical." Interesting and most valuable as the observations of Dr. Gerhard were, Dr. Wood here claims too much both for him and them. The production of "positive proof," if such an expression be permitted, is undoubtedly due to Dr. Jenner, since the tracing of the contagion of the two diseases to separate and distinct habitats is the real proof of their non-identity. With the highest appreciation of the correctness and value of the observations of Mr. Henry Kennedy, of Dublin (*Dublin Medical Journal*, 1838), we cannot allow his claim to distinction, as some writers of the sister island appear to do, to be considered as superior to that of Dr. Gerhard, Dr. Stewart, and several other physicians.

In regard to the question of duration of typhus, the following, among other facts, are given by Dr. Jenner:*

"Seven cases were received into the hospital, respectively on the fourth, fifth, sixth, eighth, tenth, twelfth, and sixteenth days of the disease, the average day of admission being the 8·7th."

Again, of thirteen patients, the exact duration of whose disease before admission could not be ascertained, the average day was about the 14·5th. We have mentioned these facts in order to render as clear as possible those relating to the duration of illness after admission, which chiefly concern our present inquiry. Now, of the seven cases first mentioned, three survived till after the fever had run its course. The others died respectively on the twelfth, seventeenth, twentieth, and twenty-seventh days of the disease, the average day of death being the nineteenth. This, Dr. Jenner observes, being probably below what a larger number of cases would give, because one proved fatal on the twelfth day from severe pneumonia. Then, of the thirteen cases, six lived some time after the termination of the typhoid fever, and the remaining seven died severally on the sixteenth, seventeenth, twenty-third, twenty-fifth, twenty-seventh, twenty-eighth, and thirtieth days of the disease. The average day of death for these seven cases being the 23·7th.

Entering into as minute details regarding certain cases of typhus, Dr. Jenner exhibits the 14·4th as the average day on which the disease proved fatal in seven cases, and then contrasts the typhus cases, of which thirty-six per cent only proved fatal after the fifteenth day of the disease, and not one after the twentieth day, with the cases of typhoid, of which 90·9 per cent. proved fatal after the fifteenth day, and nearly one-half after the twentieth day. In presenting a "summary of symptoms† distinctive of typhoid and typhus fevers," Dr. Flint observes that the duration of disease prior to admission into hospital is in a marked degree shorter in typhus than in typhoid; while in another part of his work (p. 121), he accounts, and quite correctly, for this circumstance, from the symptoms in typhus sooner assuming a degree of gravity, and so leading the patient or friends to seek hospital relief. Mild as the symptoms have been in some cases, M. Louis expressly states that he has never known the disease to continue during a less period than fourteen days.‡ The average duration of typhoid fever in severe cases, writes M. Andral, is from twenty-eight to thirty-two days, and sometimes it may continue for two months or two months and a half.§ The duration of typhoid fever according to M. Valleix,|| is very different in different cases; in the milder cases it varies from fourteen to twenty or twenty-five days; in severe cases it may extend to thirty-five or forty days, and even to a much longer period. Dr. Jackson (p. 32), in his most interesting report, previously referred to, says, "The difference as to the day of convalescence

* Monthly Journal, p. 673. 1849.

† We would here take the liberty of reminding Dr. Flint, that "Age," "Nativity," "Season," and "Duration of disease," ought not to be called "Symptoms"—the application of this term is not only confusing, but erroneous.

‡ Louis, vol. ii. p. 510.

§ Pathologie Interne, vol. iii. p. 682.

|| Guide du Médecin Practicien, vol. v. p. 464. M. Valleix has long been known as an earnest student of the subject under discussion; in the cause of which he at an early period did good service by his able analysis of the cases collected by Dr. Shattuck, of Boston.

in different years was very great, the extremes being nearly eighteen in one year, and nearly twenty-six in another." Speaking of the mode of access of typhoid fever, Dr. Bartlett observes, "There is no other acute disease, perhaps, in which the attack is more frequently slow and gradual than in this."* (p. 10.) The point we have now considered is not the one upon which the proof of the non-identity of typhus and typhoid fevers is to rest; but still, without drawing upon the reader's indulgence by quotations relative to the course of typhus, with which we shall consider him familiar, we think that in the opinions of the different authors referred to, more especially as regards the duration of the disease described, there is afforded abundant evidence of a very material difference between it and typhus.

The symptoms of typhoid fever, which we shall now shortly consider, as affording evidence of a marked difference or distinction in this form of disease from typhus, are, 1st, the appearance presented by the skin, in other words, the different character of the eruptions; and 2nd, the condition of the abdomen, the state of the bowels, and the appearances presented by the dejections. These afford the indications of greatest difference in what may be called the best marked cases, and while there exist many other, and some sufficiently striking, differential characteristics in the symptoms of the two diseases—as, for example, in the pulse, and in the condition of the chest, the aspect of the patients, and the state of the cerebral functions, the characters presented by the urine, &c.,—we shall not, for the reason already mentioned, direct particular attention to them, though it is proper here to allude to their existence. The reader, if anxious to compare the observations of different physicians on these points, is referred to Dr. Jenner's paper in the ninth volume of the 'Monthly Journal,' and to Dr. Flint's work (at page 237).

1st. *The Eruption in Typhoid Fever.*—The following is the description of the separate rose-coloured spots (the *taches roses lenticulaires* of Louis) as given by Dr. Jenner:

"The separate spots are circular, and of a bright rose colour; this hue passes insensibly at their bases into that of the surrounding cuticle. Their usual diameter is about two lines. They are somewhat elevated; but, although perceptible to the finger pressed lightly over the surface, they possess none of the seed-like hardness of the first day's eruption of small-pox, nor are they so prominent and perceptible to the touch as the papule of lichen. Their surface is rounded, lens shaped, never acuminated. No trace of vesication can be detected on their apices. If tolerably firm pressure be made on these spots, they entirely disappear; but they resume their distinctive colour and elevation as the finger is being withdrawn."†

The exact period when these peculiar spots appear varies considerably in different cases. Louis‡ mentions that, in three individuals who came under his observation on the fourth and fifth days of the disease, the spots did not appear before the sixth and seventh; further, that they were detected in six cases, who were examined for the first time on the eighth day, and that they were generally found on admission in the cases of

* The History, Diagnosis, and Treatment of Typhoid and of Typhus Fevers, &c. By Elisha Bartlett, M.D., Professor of the Theory and Practice of Medicine in Transylvania University, Philadelphia, 1842.

† Medico-Chirurgical Transactions, vol. xxxiii. p. 26.

‡ Vol. ii. p. 96.

patients who were brought to the hospital after the continuance of the disease for ten days. In all these the disease proved fatal. According to Dr. Jenner, "the eruption in typhoid fever appears from the seventh to the twelfth day of the disease, very rarely later, and still more rarely at an earlier period." Dr. Jenner has frequently noticed the occurrence of the characteristic spots, preceded by a very delicate scarlet tint of the whole skin, resembling the colour of the skin in a person who has recently left a hot bath.

"The ordinary duration of each spot," says Dr. Jenner, "is about two days, but it varies from two to six days. Fresh spots appear every day or two from the outset of the eruption, till from the twenty-first to the twenty-eighth day of disease. This successive daily eruption of a few small, very slightly elevated, rose-coloured spots, disappearing on pressure, each spot continuing visible for three or four days only, is, so far as I know, peculiar to, and absolutely diagnostic of, typhoid fever."

The trunk of the body, both anteriorly and on its posterior surface, is the ordinary site of the typhoid eruption, in comparatively few cases are the spots found on the limbs. It would appear from Louis' observations, that the abundance or rarity of the lenticular spots do not hold a fixed relation to the gravity or mildness of the disease. It is probable, from what Louis and others have found, that in a few cases of typhoid fever the distinctive eruption now described may be wanting; but it is certain that in almost all cases it will be found to exist, although its duration may frequently prove very transient, and the spots themselves be both few and indistinct.* We are sorry that we cannot congratulate Dr. Flint upon the clearness of his description of the typhoid and typhus eruptions. The spots in the former are certainly not oval; they are, on the contrary, distinctly circular. After the accurate manner in which the observations on this subject are given by Louis and others, it would be disappointing to the reader were we to dwell on the somewhat loose statements of Dr. Flint; we shall, however, indicate some of those which we think objectionable, and which, from the vagueness of statement and the slight amount of information they convey, had better have been altogether omitted.†

"In many of the cases the disease had commenced several days before they came under observation, and the eruption was apparent from the first. . . . The size of the spots is not given in the history of any case. It is stated, however, in some of the cases, that the spots were of different sizes." (pp. 88, 89.)

Absence of the eruption is mentioned as having been noted in three cases (p. 89), but no indication is given of the period when the hospital cases (two of the three) came under treatment; reference to a most important element is therefore entirely overlooked. How disappointingly vague is the following, in regard to sudamina:‡ "It is very probable that

* It should be held in remembrance that the first accurate description of the eruption in typhoid fever is due to Louis, just as the first accurate and minute description of the typhus rash is due to the Irish physicians.

† In the following quotations from Dr. Flint's work the italics are our own.

‡ The eruption of sudamina, it may be observed, is more frequent in typhoid than in typhus fever. "The elimination of lactic acid from the skin is more permanent and distinct in enteric fever than in typhus. The skin also acquires a very peculiar smooth, satiny feel, and sudamina are more profuse, and desquamation more copious and remarkable, in this disease than in the

they may have been present in other cases in which they were not noticed." (p. 89.) A *petechial* eruption, which, not unlikely, for no further account of it is given, was caused by flea-bites, is stated to have been present in one case where the characteristic spots were absent (p. 89). Another case, equally remarkable, presented "a combination of the eruptions" of typhoid and typhus. We might comment on several other descriptions, such, for example, as a case in which "some of the spots were vesicular, the contents of the vesicles being subsequently absorbed;" but in the treatment of a work in which there is so much to commend, and by which its author has proved himself a most earnest and successful student, we shrink from the appearance of hypercriticism.

We need not do more than direct attention to the differences presented by the eruption of typhus, as compared with the description now given of that in typhoid. Dr. Jenner has not unaptly designated the eruption of typhus, "the mulberry rash;" and has further described it as consisting of "very slightly elevated spots, of a dusky-pink colour." The general distribution of the typhus eruption, very frequently covering the chest and abdomen, and often being distinctly visible on the face, the legs and feet, the arms and hands, the toes and fingers, forms a striking contrast with the usually very limited distribution of the rose-spots in typhoid fever.*

According to Dr. Jenner, in forty-three cases of typhus analysed by him, the eruption underwent certain changes in the course of the disease.

"In one, two, or three days, the spots were no longer elevated above the surrounding cuticle; their hue was darker and more dingy than on their first appearance; their margins rather more, but still imperfectly defined; and now (instead of disappearing completely on pressure, as at first) they only faded on pressure."

Dr. Jenner has observed a further change, which he calls the third stage, in it: "The centres of the spots became dark purple, and remained unaltered by pressure, although their circumferences still faded; or the entire spots, the circumferences as well as the centres, changed into true petechiæ." Now the importance of these distinctions, the fruit of Dr. Jenner's most careful observations, is at once seen, when we consider that by them is exhibited the real difference between the typhoid and typhus, no such changes as the spots in the latter undergo, occurring to those of the former. Dr. Jenner truly says, that on the first day of the appearance of typhus spots, the most tutored eye might be in some doubt as to which order they belonged; but when, after a day or two, some or all of the spots passed into the second stage, the disease of which they were characteristic was proclaimed. So much for the distinctive appearances presented by the eruptions of the two diseases. We are aware of no extended observations which have been published corroborative of Dr. Jenner's; but, on the other hand, we know none which oppose them, while our own limited experience is entirely in accordance with his views.

We have now to consider, secondly, the condition of the abdomen, the

other."—Dr. Charles Ritchie, of Glasgow, in the *Monthly Journal* for 1846-47, p. 254, whose careful researches, published in that periodical and in the *Glasgow Journal of Medicine* for 1854, have materially added to our knowledge of the generic distinctions of typhus and typhoid.

* *Monthly Journal*, vol. ix. p. 677.

state of the bowels, and the character of the dejections, in typhoid fever, as affording marked distinction between that disease and typhus. So far as symptoms go, it is here that we naturally look for the most complete evidence of that distinction. The names of abdominal and enteric fevers express in a word the character of the disease, which has been regarded as the most expressive. Tenderness of the abdomen, if not a constant symptom in typhoid fever, is at all events a very frequent one. Dr. Jenner found it present in three-fourths of the cases in which accurate examination was made.* In a great number of cases, the tenderness, if not limited to a particular part of the abdomen, the right iliac fossa, is most marked there, and is generally associated with a peculiar gurgling sound when pressure somewhat suddenly is made. In regard to abdominal tenderness and gurgling, Dr Flint writes:

“Of the 18 hospital cases, more or less tenderness was present in 11. The tenderness was either moderate or slight in all but 3 cases. It is noted as especially marked in the right iliac region in 2 cases, and in both iliac regions in 4 cases.

“*Gurgling*.—Of the cases of typhoid, this symptom was present in 12, and its absence was noted in 2 cases.”† (p. 85.)

Again, in his summary of symptoms distinctive of typhoid and typhus fevers, Dr. Flint, in speaking of tenderness on pressure over the abdomen, says, this is “an almost constant symptom in typhoid, and less frequently present in typhus. In the latter, usually slight; and in the former, more apt to be marked, or considerable in degree.” (p. 237.)

Dr. Jenner has drawn attention to the peculiar tub-shaped form presented by the belly, which he ascribes to the flatus, which is often in very considerable amount, occupying the ascending, descending, and transverse colon.

In regard to the state of the bowels, Dr. Flint thus sums up the evidence he has collected:

“*Diarrhœa* present in one-half of the cases of typhoid, and in one-third of the cases of typhus; in the latter type always mild or slight, but in the former sometimes prominent as a symptom. Hæmorrhage from the bowels, characteristic of typhoid.” (p. 237.)

The tendency to looseness of the bowels, whether arising spontaneously or induced by very slight doses of laxative medicine, is a symptom of typhoid fever upon which it is unnecessary here to enlarge. The character of the stools is thus alluded to by Dr. Jenner:

“The consistence of the stools was watery in twelve cases at some period of the disease, and soft, pultaceous, or almost fluid, in four others. In eight cases only were stools of natural consistence passed, after the patients came under observation, and in five of these the stools were watery during some period of the disease. Their colour varied from pale brown to almost black; when watery they were usually pale, yellowish brown,‡ with a sediment composed of small, solid, yellowish particles.”

* In fifteen out of twenty: in one of the fifteen, however, it did not arise till peritonitis, the consequence of perforation, had taken place. *Monthly Journal*, vol. ix. p. 820.

† In what proportion of cases this was, cannot be gathered, as Dr. Flint adds, “in the remainder, nothing is said on this point.”

‡ A colour owing to which the name of “pea-soup stools” has been not inappropriately applied.

In regard to the occurrence of bloody stools, Dr. Jenner further observes, that seven out of twenty-one patients, particulars respecting whose stools were recorded, passed blood from the bowels; whereas "hæmorrhage from the bowels did not occur in a single case" of typhus.* The really important point connected with the occurrence of these abdominal symptoms during life, is the appearance presented by the intestines, more particularly the glands of Peyer and the mesenteric glands, and not unfrequently associated with this, the deposit of a peculiar exudation in the spleen, and more rarely in other organs. Into the examination of the morphological changes undergone in these organs we do not here propose to enter, for the reason previously assigned. But from the data afforded by our two authors we shall draw attention to the frequency of the intestinal disease in typhoid fever as contrasted with the usually unaffected condition of the bowels in typhus.† In twenty-three fatal cases of fever which Dr. Jenner had an opportunity of examining, the agminated glands, or Peyer's patches, were ulcerated in all. In forty-three fatal cases of fever (typhus), recent disease of Peyer's patches was absent in every one. In regard to the affection of the mesenteric glands, in the same cases of typhoid fever, twenty-three in number, these organs were more or less extensively diseased in all. In the cases of typhus, with two exceptions in which they were the seat of tubercular deposition, the mesenteric glands presented no deviation from a healthy structure. Dr. Flint's remarks on the affection of the bowels are few in number, and are not, we regret to say, capable of conveying a very correct idea of what he did observe.

"Of the fatal cases," writes Dr. Flint, "the bodies were examined with particular reference to the lesions deemed to be characteristic of *typhoid fever* in *terce*. Of these cases, *nine* had been classed under the head of *typhoid*, judged by their symptoms, and *three* under the head of *typhus*. On reference to the description of the morbid appearances in these cases severally, it will be seen that in each of the *nine* typhoid cases there existed notable changes in the intestinal follicles, accompanied by corresponding degrees of enlargement of the mesenteric glands."

Dr. Flint then proceeds to describe more minutely the follicular lesions; and having done so, makes the following remarks on the appearances presented in the *three typhus* cases:

"In each of the three *typhus* cases, on the other hand, there existed changes in the follicular patches, accompanied in *two* cases by very slight enlargement of the mesenteric glands. The follicular lesions, however, were insignificant in comparison with those observed in the *typhoid* cases. The patches were simply developed so as to be visible. They were slightly hypertrophied, not projecting several lines above, or depressed below the level of the mucous surface, as in the *typhoid*

* The condition of the urine in typhoid fever has been made a particular subject of study by Dr. Q. W. Edwards, and to his interesting paper in the Monthly Journal for September, 1853, we beg to refer the reader. See also, on this subject, a short paper by Mr. Trotter, in the Lancet, for 1854. In 1852, the writer of this article expressed the opinion, founded on personal observation, that albuminuria would be found a frequent concomitant of typhoid fever, and specially occurring in the advanced stages of the disease. See Monthly Journal for 1852.

† It must be observed (and all who are familiar with Dr. Jenner's original papers will remember), that the affection of the agminated glands was the feature by which he distinguished the typhoid from the typhus cases, and therefore the facts he brought forward, and which are mentioned above, are not to be looked upon as affording any new ground of distinction, seeing that the existence of the intestinal lesion was observed by Dr. Jenner before the cases of typhoid fever were classed as such.

cases, and in no instance presenting an ulcerated appearance. The contrast, indeed, is scarcely less striking than if, in the typhus cases, the follicles had remained invisible. The united number of observations, thus, which are contained in the preceding reports, sustain the existence of characteristic lesions of the intestinal follicles and mesenteric glands in typhoid fever; but they also show that these parts do not always continue wholly unaffected in *typhus*." (pp. 240, 41.)

If they do not, and if Dr. Flint's three post-mortem examinations reveal that they did not, we must confess that the proof he has brought forward to establish this fact is altogether inadequate. Dr. Flint must have forgotten that Peyer's patches are not such invisible things as he here appears to regard them. It is by no means uncommon to see them clearly marked in persons who have died from diseases altogether unconnected with the intestines, and as a general rule in young subjects (and the mean age of Dr. Flint's typhus cases is stated at $26\frac{1}{2}$) the agminated glands are distinctly recognisable. We cannot then agree with Dr. Flint, but, on the contrary, think that the facts he has adduced fail in showing "that these parts do not always continue wholly unaffected in typhus."

Having thus briefly adverted to the chief differences in the course, symptoms, and lesions of typhoid and typhus, it remains for us to direct attention to the view which has been offered by Dr. Jenner, from inquiries as admirably planned as they have been efficiently carried out, of the essential difference in the specific cause of typhoid, typhus, and relapsing fevers.

We have not thought it necessary to offer any detailed account of the course and symptoms of relapsing fever. It is thus sketched by Dr. Jenner:

"Sudden rigors, headache, skin hot and dry, tongue white, urine high-coloured, bowels regular, occasional or frequent vomiting, loss of appetite, absence of abnormal physical abdominal signs. In severe cases, jaundice, profuse sweating on about the seventh day, followed by apparent restoration to health; on, from the fifth to the eighth day, reckoning from the apparent convalescence, repetition of the original symptoms, with greater or less severity, again terminating in sweating, and then permanent convalescence."

To this may be added the frequent occurrence of epistaxis at the period of crisis.

In Edinburgh a second relapse was not unfrequent, and even a third has been observed. This form of fever has been twice epidemic in Edinburgh during recent years—in 1843-44, and 1846-47.* The probable existence of this form of fever during upwards of a century is shown by Rutton in his 'History of the Diseases of Dublin,' who describes a non-malignant fever of six or seven days' duration, terminating in a critical sweat, but followed by one, two, three, in some cases even four, relapses—yet recovering. In 1800 and 1801 there was an epidemic of a similar fever in Ireland. Barker and Cheyne, in their Reports, and Dr. Welch, in his work 'On Bloodletting,' prove the existence of a similar fever in 1816, 1817, 1818, 1819, and 1820; while Dr. Christison shows the pro-

* See Dr. Cormack's Account of the First Epidemic. London, 1843. Also Dr. Halliday Douglas in Northern Journal of Medicine, for 1845; and for account of the latter see Dr. William Robertson, in Monthly Journal, for 1848; and Dr. Robert Paterson, in the Edinburgh Medical and Surgical Journal for the same year.

bable identity of the fever he so carefully studied in 1826 with the fever described by Welch.*

The following are the facts, summarily expressed, of Dr. Jenner's induction, which we shall presently notice:—In the year 1848,† one-fourth of the cases admitted into the Fever Hospital had typhoid fever; while from 34 foci of typhus fever, yielding 101 cases, there was brought to the hospital once only a case of typhus fever and a case of typhoid fever from the same house; and during the same time, among five localities, affording 9 cases of typhoid fever, one locality only—viz., the house from which a father and son were brought—yielded a case of typhoid and typhus fever. In 1849, although eighteen foci of typhus fever yielded 51 cases, and four foci of typhoid fever afforded 10 cases, not a single example of the two diseases being received into the hospital from one house occurred. The facts of the exceptional case, which happened in 1848, were the following:—A man, aged 46, was admitted October 10th, 1848, with well-marked typhus fever, while his son, aged 16, who had been received into the hospital on September 19th, laboured under equally well-marked typhoid fever. The mother of the boy, however, had visited him in the hospital, and therefore might have carried the contagion of typhus fever to her husband. The father, moreover, had been little exposed to the contagion emanating from the son, because the latter, a vagabond, at variance with him, was from home when the father was taken sick. We feel more disposed to agree with Dr. Jenner in the following observation in regard to such exceptional cases as the one detailed, than to attach much weight to the circumstances either of the mother visiting the boy in the hospital where there were typhus cases, or, though we regard it as of more importance, the father being little in the company of his son. Dr. Jenner holds that exceptional cases “must be met with more frequently than similar exceptional cases are met with in diseases having a specific cause, universally acknowledged to be different,” before they can be of any value in proving the identity of typhus and typhoid fevers. It has occurred to Dr. Jenner, within three years, to see a case of typhus brought to the hospital from a house where all the children were suffering from measles. Another case of typhus, brought from a house in which the children had scarlet fever—a girl admitted with scarlet fever who had been on terms of intimacy with another girl, admitted shortly before with typhoid fever.

In April, 1849, a girl suffering from relapsing fever was brought from a house in Fulham; in a few days her brother and her sisters were admitted into the hospital. At this time typhus was the prevailing disease, and typhoid was much more widely spread than relapsing fever, but all had the same fever.

Judging, then, from such facts, Dr. Jenner arrives at the conclusion—and we confess that, with the confidence we have in the accuracy of his observations, we feel committed to the same—that the specific causes of typhus, typhoid, and relapsing fevers are absolutely different from one another:

“If,” says Dr. Jenner, “small-pox be separated from measles, and both from

* See Dr. Jenner's *Gulstonian Lectures*, p. 43; the *British and Foreign Review* for 1851; and Dr. Wood's *Lectures*, vol. ii. 361.

† *Medico-Chirurgical Transactions*, vol. xxxiii. p. 40.

scarlet fever, because their course, symptoms, lesions, and specific causes are different, so must, for like reasons, typhoid fever, typhus fever, and relapsing fever be separated from each other, and regarded as absolutely distinct diseases, not merely varieties of each other, as scarlatina anginosa and scarlatina sine eruptione, but distinct species of disease, as are scarlatina, rubeola, and variola."

The historical bearings of the subject we have considered, deeply interesting as they are, being altogether too immense for our present consideration, we beg to refer the reader specially to Dr. Stewart's admirable paper for a key to its vast literature; and to the very able articles on fever in this Review for 1841 and 1851, as well as to the last chapter in Dr. Jenner's 'Gulstonian Lectures.' It must not be forgotten that the authors of three countries—of France and America, besides our own—have added imperishable monuments to their talents and labours in the cultivation of this most interesting subject. Of the researches of Dr. Flint, the latest American observer, as embodied in the work we have had under our notice, we are very glad to express a high opinion. Not free from confusion in some particulars, from causes its author indicates in his preface, Dr. Flint's Report contains a body of most useful facts, and for the most part very accurate deductions from those facts, which are equally honourable to Dr. Flint and to the medical school of which he is so distinguished a teacher.

J. Warburton Begbie.

REVIEW X.

The Obstetric Memoirs and Contributions of JAMES Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, &c. Edited by W. O. PRIESTLEY, M.D., Edinburgh; and HORATIO R. STORRER, M.D., Boston, U.S. Vol. I.—*Edinburgh*, 1855. pp. 857.

THE republication of the obstetric writings of Dr. Simpson in a complete and accessible form was an undertaking no less due to the reputation of their author than to the interests of obstetric science. Few men have laboured more assiduously in this department of medicine, and few have contributed more largely to its scientific advancement by the publication of various original papers and practical observations. It was right that the results of such labours should be rescued from the perishable record of the various serial publications in which they originally appeared, and be presented in an authorized and collective form to the notice of the profession. The present volume supplies, to a certain extent, this desideratum; and without assuming that all the views which are contained in it will receive general assent, believing rather that many are both unsound in doctrine and unsafe in practice, we are yet convinced that it will be received as a valuable boon by the profession, and consulted by all who are interested in the progress and advancement of obstetric science.

The subjects treated of in the present series are arranged under three heads: the first comprising forty-three papers On the Special Pathology of the Unimpregnated Female; the second, five upon the Physiology and Pathology of Pregnancy; and the third, thirty-three On Natural and Morbid Parturition. It should be observed that many consist of a few curt and extempore observations only, whilst others are extremely elabo-

rate. As it would be impossible to bring all the topics thus treated of before our readers, within the necessary limits of this article, we propose to confine ourselves to an examination of some of the more important papers, selecting those points which especially distinguish the views and practice of their author.

In the first, the subject of the general diagnosis of uterine disease is very fully considered, and this is well worthy of attentive perusal, not only because all the elements of such diagnosis are very lucidly set forward, but also because it furnishes a clue to many of those views respecting uterine pathology upon which so much of the author's peculiarity of practice is founded. He treats of the subject under two heads—the first comprising the *symptoms*, and the second the *signs* of uterine disease; the former are referred to the five following sources or varieties of information:

1. *Derangements in the Functions and Vital Condition of the Uterus itself*, as indicated by the quantity, character, periodicity, &c., of the menstrual and mucous secretions of the organ; by the occurrence of morbid uterine or vaginal discharges—as blood, serous fluid, pus, &c.; by the existence of morbid sensations in the region of the uterus—such as different modifications of pain, intermittent or continuous, feelings of heat, weight, tension, bearing down, &c.; and if the patient be married, by the reproductive powers being affected, as shown by sterility, the recurrence of abortions, premature labour, and the like.

2. *Dynamic Symptoms in neighbouring Pelvic Organs*—more especially affections of the rectum and bladder, and of branches of the vessels and nerves passing through the pelvis. In this division are included pains about the bladder or rectum, in the groins, along the crest of the ilium, and along the course of the crural and sciatic nerves; intermittent pains in the lower part of the abdomen; derangement of the functions of the rectum or bladder, producing either constipation, or difficult or painful defæcation on the one hand, or too frequent micturition, dysuria, retention or incontinence of urine, on the other.

3. *Sympathetic Pains in different and distant Parts of the Body*—including pain in the mammæ, along the lower extremities; in the loins, and at points along the course of the spinal column; in the parietes of the thorax or abdomen; on one or other side, and especially under the left breast; along the course of the colon; under the margin of the ribs; increased by any causes which tend to an increased action of the uterus, such as the erect position, menstruation, &c.

4. *Derangements of Functions in Distinct Organs*, including affections of the kidneys, intestinal canal, liver, lungs, nervous system, and skin.

5. *States of General Constitutional Derangement*.—These comprise more particularly febrile, cachectic, and anæmic conditions.

The above may be regarded as the principal symptoms, local, sympathetic, and constitutional, of uterine disease; and the whole as comprising its chief symptomatology in the widest acceptation of the word. But there is this evident error or omission in the review of these morbid phenomena, that no attempt is made to determine their absolute relations to the diseases in question—whether, in reality, they are specifically symptomatic of primary disease of the uterus, or of abnormal conditions

affecting distant parts, or the economy at large, of which the uterine affection is but a secondary and subordinate feature. That the symptoms in question are very commonly associated with uterine disease we do not doubt; but that they are specifically its consequences in all cases, is a point we are prepared to contest. We have devoted some time to the determination of the relations which subsist between uterine ailments and various remote and constitutional affections, and the result has convinced us that the supposed symptoms and consequences of uterine disease are in many cases to be regarded rather as their causes or antecedents. We would instance in particular morbid states of the nervous system, the blood, and the digestive organs; and we are fully persuaded that the category might be extended. The analysis we have therefore given of uterine symptomatology, founded upon Dr. Simpson's views and experience, must be understood as comprising associated rather than contingent derangements, and a wide range of morbid actions not necessarily dependent upon the uterine ailment.

The signs of uterine disease, or rather the means by which we are enabled to discriminate or detect them, are given in the following summary of the author's:

1. The external or abdominal examination of the patient by sight, touch, auscultation, and percussion.
2. The tactile examination of the uterus, ovaries, &c., by the vagina or by the rectum.
3. That most important mode of diagnosis, viz., the simultaneous combination of the external and internal modes of tactile examination.
4. The use of the speculum.
5. The use of the uterine sound.
6. The use of sponge-tents, with the view of dilating the os uteri, so that the finger can be introduced into the cavity of the cervix or cavity of the body of the organ.
7. The microscopic and chemical examination of the discharges from the uterus and vagina.
8. The employment of the exploring needle in cases of fluid collections, in order to ascertain the contents of such collections; and,
9. The adoption of anæsthetic agents to relax the abdominal parietes, and enable us to practise the different modes of examination, in cases of excessive or neuralgic tenderness of the abdominal surface or vagina, &c.

We believe, as stated by the editors, that the profession is indebted to Dr. Simpson for the introduction of four of these means of diagnosis, viz., the sound, sponge-tents, the exploring needle, and the production of anæsthesia to facilitate examination; and we advert to the circumstance as showing the extreme, and as it seems to us undue, importance attached by him to this mode of investigation—a circumstance which is indeed attested by the general tenor of the paper before us. We are, for instance, repeatedly reminded of the uncertainty and insufficiency of rational symptoms in the discrimination of uterine disease. We are told that by these means we may indeed determine its existence, but not its nature; that by physical exploration alone this object can be attained; that improved physical means of diagnosis are still so much required; and that it is only through these that our knowledge of uterine pathology

can be really advanced. It is not difficult to perceive that an undue preference for physical means of diagnosis should lead to a similar preference for physical methods of cure; and thus we may deduce the origin and introduction of those peculiar mechanical principles of treatment which constitute the leading characteristics of the school of which Dr. Simpson is the acknowledged head, and in regard to which professional opinion has of late been so much divided.

But whilst we concede the importance of carefully investigating the physical changes of an organ affected by disease, or, in other words, the value of physical diagnosis, and would neglect no means by which this object can be safely and efficiently accomplished, we would yet observe, that there are some limits to its application and utility. It is quite true that by physical means we may become readily acquainted with the physical changes which have taken place in an organ, but what we desire to attain to for the purposes of accurate treatment, is not so much a mere knowledge of the lesions of structure which have been effected, as a knowledge of the conditions under which they arise, their course, tendency, and relations to coincident phenomena. When, for instance, with the aid of the speculum or uterine sound, we have discovered the existence of an excoriated cervix or a retroflexed fundus uteri, we have in reality but commenced our inquiry, and without carrying our investigations further, very erroneous views will result both in regard to doctrine and practice. Thus, from the frequent coincidence of redness with abrasion of the cervix, it was assumed that the latter lesion was necessarily of an inflammatory character—was spoken of as inflammatory ulceration, and said to require the usual treatment for inflammation; whereas it can be shown that the lesion in question not only occurs without any redness, or any other sign of inflammation, but would be aggravated by a resort to treatment adapted for inflammatory conditions. So also it was assumed, from the frequent coincidence of this lesion with various functional derangements of the uterus and disordered states of the general health, that it stood in the relation of cause to these affections; whereas it may be shown that such lesions do very commonly exist, not only without any remarkable derangement of the functions of the uterus, but also without any unfavourable reaction upon the general health. Again, with reference to the discovery of a retroflexed condition of the uterus, it by no means follows that we are thence enabled to deduce its real nature and pathology; and accordingly, under erroneous impressions, a practice has sometimes been adopted altogether at variance with sound physiological and pathological doctrines. We could extend the number of examples to show that there is a limit to the value of mere physical diagnosis, and that the aid of dynamic or rational symptoms must be brought to our assistance before we can attain to a right knowledge or apprehension of the nature of the lesions thus revealed.

The tendency of modern researches in uterine pathology, based almost entirely upon physical evidence, is forcibly and accurately depicted by Dr. Simpson himself; and his remarks on this subject form a singular corollary or contrast to his oft-repeated maxim, that "The medical science of the present day, owes its superiority over that of an earlier date, to no

circumstance more than to the increased attention that has for a considerable time past been directed to the study and improvement of physical diagnosis," and that it is from this that so much is to be expected.

"Since the diseases of the uterus and its appendages have of late years attracted so much more the attention of the profession than they formerly did, one grievous error," he observes, "has been committed in their study and investigation. The error I allude to is the error of exclusiveness. Formerly many practitioners seemed to look upon all diseases of the uterus as diseases indicative of debility; and they treated almost every one of them with muriate of iron and other chalybeate preparations, sometimes adding, where there was any discharge, the local employment of astringent or tonic injections. I fear that even yet you will find some old practitioners treating their uterine cases upon this sole principle. Then a second set of pathologists would, if we may judge from their writings, seem to suppose that all diseases of the uterus are marked by, if they do not consist of, congestion and engorgement of blood; and that they are to be remedied by the remedies applicable to their state. A third set, again, look upon the general run of uterine cases as almost invariably inflammatory in their nature, and imagine that we are sure, or almost sure, to find in every case inflammation, or some of its results, as ulceration, purulent discharges, &c. A fourth set would seem to fancy all uterine ailments to be produced by some mechanical displacement or dislocation of the uterus, and to consist of prolapsus, versions, and flexions of this organ upon itself and upon the neighbouring parts. Again, there are some practitioners—one in particular, in immense practice in America—who would appear to believe that the affections of the uterus are fundamentally nervous or neuralgic disorders, and that they are always to be treated by the local inunction upon the cervix uteri of morphia, aconite, and similar sedatives. Another section of pathologists imagine the so-called uterine diseases are, after all, not uterine, but ovarian; and that ovarian irritation and inflammation is actually the source and origin of much of the suffering that is imagined to be uterine in its seat. Lastly, for it is needless to extend this tedious enumeration, you will find another set enjoying the belief, that these supposed uterine or ovarian diseases are not at all uterine or ovarian in their origin, but in reality diseases of the general system; they may not deny that local affections do occur in the uterus and ovaries, but these local affections are, in their opinion, results and effects of some more general or constitutional disease of the nervous system or of the economy at large." (pp. 3, 4.)

Let it be added, that these remarks apply to doctrines which have become rife since the physical exploration of the uterine organs has been so much resorted to by medical men, and if true, they are calculated to shake our confidence in the sufficiency of the means to the end proposed; to show that its value may be over-rated and its revelations misinterpreted; and that the mere recognition of facts, without a just perception of their practical relations, can add little but doubt and confusion to our existing knowledge of uterine disease.

From the very elaborate memoir on the uterine sound, we extract the following summary of the chief purposes which, in the opinion of our author, the instrument is calculated to fulfil:

1. The sound increases to a great degree our power of making a perfect and precise tactile examination of the fundus, body, and cervix of the uterus, by enabling us to bring these portions of the organ successively into the most convenient position for external or internal examination or manipulation;

2. The previous introduction of the instrument facilitates and simplifies the subsequent visual examination of the cervix uteri with the speculum

by lessening the difficulty which sometimes arises of catching the os and cervix uteri in the upper or internal extremity of the instrument;

3. By its use we may, in many instances of pelvic and hypogastric or abdominal tumours, ascertain the connexion or non-connexion of these tumours with the uterus. Thus, when the tumour is uterine, the instrument, when passed into the uterine cavity, enters, as it were, more or less into the very structure of the morbid mass, and the tumour and instrument afterwards reciprocally move in exact correspondence with each other. When, however, the tumour is not uterine—(1.) the uterus may be retained in its situation with the sound, and then, by means of the hand above the pubis, or by some fingers in the vagina, the tumour, if unattached to the uterine tissues, may be moved away from the fixed organ; or (2.) the tumour being left in its situation, it may be possible to move away the uterus from it to such an extent as to show them to be unconnected; or (3.) instead of keeping the uterus fixed and moving the tumour, or fixing the tumour and moving the uterus, both may be moved simultaneously, the uterus by the bougie, and the tumour by the hand or fingers, to opposite sides of the pelvis, to such an extent as to give still more conclusive evidence of the fact;

4. The uterine bougie is capable of affording valuable diagnostic information, by enabling us to measure the length of the uterine cavity, as in the following cases, in which it is abnormally increased: (1.) Morbid permanence of the state of puerperal hypertrophy; (2.) Normal elongation of the puerperal uterus as a sign of delivery; (3.) Increased length in metritic and congestive hypertrophy of the body of the uterus; (4.) Longitudinal hypertrophy of the uterus, and especially of the cervix; (5.) Enlargement of the uterus and uterine cavity from the growth of fibrous tumours in the parietes of the organ; (6.) Enlargement and distension of the uterus from polypi; (7.) Elongation of the uterus in hernia of the organ. The uterine bougie affords information in the following cases, in which the uterine cavity is abnormally diminished: (1.) Preternatural shortness of the uterus from original malformation; (2.) Shortening of the uterine canal from stricture or partial obliteration; (3.) Diminution of the depth of the uterine cavity from inversion of the organ.

We have endeavoured to condense into the preceding paragraphs the more important uses of this instrument, as set forward in the text; but we are bound to add that so far as our experience of it has extended, it has not altogether realized the expectations we had formed of its value. We do not, however, participate in the opinions of those who regard it as a useless and a dangerous instrument, and would discard it altogether from practice. We have no doubt that judiciously employed, it is useful in the diagnosis of many obscure forms of uterine disease; but at the same time we are equally persuaded that its employment requires much care, and that the range of its utility is more limited than that claimed for it by the author. In our own practice we have experienced the following difficulties in its employment:

1. Although great experience may give facility in the introduction of the instrument, it is yet an operation by no means so easy of performance as is commonly represented. In cases in which the cervix is high up, as also in those in which the os uteri is small, or the vagina constricted, we have found great difficulty in its introduction by merely manual pro-

ceedings. In the first of these cases we have found it difficult to insert its point into the os uteri when guided by a single finger, on account of the mobility of the organ; and when two fingers have been passed into the vagina, for the purpose of steadying it, we lose the guide which is afforded by the application of a single finger to the os uteri. Under these circumstances we have found it more convenient to bring the cervix into view by means of the speculum, and to introduce the instrument by the aid of vision, rather than of touch. In this way there is seldom any difficulty in introducing it; and by varying the axis and direction of the speculum, so as to follow the necessary movements of the instrument, we have been enabled to employ it with considerable advantage;

2. We have occasionally experienced some difficulty in passing it into the body of the uterus after it has entered the cervix, either from some constriction at its distal extremity, or from its becoming entangled in the lacunæ or rugæ of the lining membrane of the cervix. We apprehend that in such case it must be difficult to determine the exact cause of the arrest, whether, for instance, from obstruction, constriction, or inflexion. In any case, however, it must be obvious that violence would be unjustifiable, and hence we prefer to forego the advantages of the instrument, and to be satisfied with negative evidence, rather than incur the risk of injuring or wounding the interior of the uterus;

3. We have met with cases in which the most careful use of the sound occasioned extreme pain and suffering, and was followed by persistent hæmorrhage. Other dangers, such as contusion, laceration, and injury of the uterine parietes, have been known to follow its incautious or forcible employment; and therefore, without any wish to depreciate its value, we feel it necessary to qualify our recommendation of it with the warning that great care and judgment are necessary in its employment.

Passing over several minor papers upon the Use of the Exploring Needle in the Diagnosis of Doubtful Forms of Pelvic and other Tumours—Anæsthesia as a means of Diagnosis—Inflammatory Eruptions upon the Mucous Membranes of the Cervix Uteri—Medicated Pessaries—Chloride of Zinc in Ulceration of the Cervix Uteri—Potassa Fusa in inflammatory Induration of the same organ (which we regard as very questionable practice)—Morbid Deficiency and Morbid Excess in the Involution of the Uterus after Delivery; and two papers upon Fibroid Tumours of the Uterus, we come to three interesting communications upon Polypi of the Uterus, of which the principal points mooted are the following:

1. The employment of the uterine sound as a means of diagnosis in the case of polypi growing from the lips of the cervix uteri. It being contended that by this we can best determine the position of the os, and the direction of the uterine cavity to the abnormal growth; and although it is admitted that the use of the instrument is difficult, and requires considerable care in these cases, yet it is believed to be capable of furnishing information which would amply repay the overcoming of any difficulties which might be met with in its employment.

2. The employment of sponge tents for the mechanical dilatation of the cervix uteri in cases of intra-uterine polypi to such an extent as to admit of the introduction of a finger into the uterine cavity, for the purpose of exploration and diagnosis, a practice which is very fully described and illustrated; and,

3. The advantages of the excision of large pedunculated uterine polypi over deligation; which the author regards as being, in many respects, a safer operation, one which upon the whole is more easily performed, the cure by it being infinitely quicker, and admitting of being accomplished with far less restraint and annoyance to the patient, with less risk of local irritation, and with less ultimate chance of actual peril to health and life.

With regard to the general questions raised in these very practical papers, we would venture to express our opinion, that the suggestion of the employment of sponge tents for the purpose of exploring the interior of the cervix and body of the uterus in certain cases, is one of great value, the operation being comparatively harmless, and for the most part unattended with much pain or suffering. In giving the symptoms of intra-uterine polypi, the author enumerates menorrhagia, recurrent, almost constant, and periodical; leucorrhœa, of a mucous, purulent, or serous character; increased size of the cervix and body of the uterus, from the presence and distension of the polypus, and sympathetic irritations in the bladder, rectum, and distant organs; but he omits to mention what we believe is often an important symptom of the disease—viz., periodical pains or contractions of the uterus, not dissimilar to those that occur in abortion, and which are evidently occasioned by the efforts of the uterus to get rid of the abnormal body in its interior. He also omits to make any reference to the employment of galvanism, either for the purpose of stimulating the uterus to expel the polypus, or of moderating any attendant hæmorrhage. We are aware that Dr. Simpson's trials of galvanism, for the purpose of exciting uterine action during labour, were not such as to lead him to think favourably of its powers. This, however, is a question to which we shall hereafter revert, and in the meantime we would remark, that we have strong grounds for believing that it may be usefully employed in the treatment of polypus uteri for the fulfilment of both of these indications.

On the subject of the excision of large polypi, we would observe, that whilst concurring with the author in a full appreciation of the dangers to be apprehended from sloughing and suppuration consequent upon deligation, we should yet feel equally apprehensive in regard to the dangers of hæmorrhage as a probable consequence of excision. Dr. Simpson very justly draws attention to the liability of phlebitis and surgical fever supervening upon the use of the ligature, as a consequence of the absorption of the ensuing purulent discharges. But it is to be remembered that an empty or exsanguine state of the bloodvessels powerfully predisposes to the same results; and, therefore, in taking a comparative estimate of the merits and demerits of these respective operations, the fact must not be lost sight of, that the conditions in question very commonly occur in an inverse ratio in the two, and that the greater risk of hæmorrhage after excision forms, in some degree, a set-off to the increased suppuration and sloughing after deligation; whilst the dangers of the latter may be somewhat obviated by the use of antiseptic injections. We are certain that the liability to phlebitis, puerperal fever, and various secondary affections, is infinitely greater in those cases in which profuse hæmorrhage has followed upon mechanical injury, or difficulty during

labour, than in those in which, without it, the same amount of injury had been received, and accordingly we have felt it necessary to watch cases with greater vigilance whenever this circumstance has occurred. Believing, then, that the operation of excision, as ordinarily practised, is fraught with great danger, from the risk of consecutive hæmorrhage, but otherwise is far safer and better than that of deligation, we would wish to consider whether it might not be practised in a manner that would obviate this consequence. We have no practical facts to offer on the subject, but have been led to think that some modification of the electric cautery might be substituted for the knife, or that the object might be attained by the torsion of the polypus previously to its division. It is quite certain that the risk of hæmorrhage is the great objection to excision, and that any plan of proceeding by which this might be obviated or lessened, would add very considerably to the safety of its performance; until this desideratum however has been attained, we fear that the advantages of the excision over the deligation of large uterine polypi will be found to be less considerable than that claimed for it by Dr. Simpson.

Two papers follow upon Amputation of the Cervix Uteri. In the first, the history of a case of cauliflower excrescence from the os uteri is given, in which the operation was successfully performed, and to it are appended some remarks upon the pathological nature of the affection. In the second, three cases are reported, in which excision of the cervix uteri was performed, and these are followed by some general remarks upon the operation in carcinomatous disease. The subject of malignant disease of the uterus is further illustrated by two additional papers—the one Upon the Occasional Latency of the Symptoms in Advanced Carcinoma Uteri, and the other On Carcinomatous Disease of the Cavity, Body, and Fundus Uteri. We will commence the consideration of this very important series by an examination of Dr. Simpson's opinions respecting the pathological nature of the cauliflower excrescence.

“The pathological nature of this variety of morbid growth,” he observes, “has given rise to a considerable difference of opinion among physicians. Drs. Gooch, Hooper, Davis, and Lee regard it as truly cancerous in its character; others—as Drs. Clarke, Burns, and Waller—consider it as a morbid tissue, not necessarily of a malignant or carcinomatous nature. A number of circumstances appear to me to show that, in reference to at least the first stage of cauliflower excrescence, the opinion of these latter authors is probably correct. The occurrence of the disease in some cases as early as the twentieth year of life; its occasional shrinking and almost total disappearance upon the application of a ligature, or after death; the frequent slowness of its general progress during life; the apparent absence of diseased deposits in the neighbouring tissues and parts upon the dead body; and, above all, the alleged restriction, and even complete removal, of the tumour, in one or two instances, by the use of astringent applications and other simple means, form so many circumstances strongly pointing to the opinion that, in the earlier part of its progress, the tumour cannot be regarded as of a carcinomatous character.” (p. 168.)

The view thus expressed of the non-malignant nature of the disease is, however, somewhat at variance with the following remarks, which occur almost immediately after the preceding:

“But whatever view we may take of the primary nature of the cauliflower excrescence of the cervix uteri, we have sufficient evidence for believing either that

this disease has been often confounded with carcinomatous or medullary fungus from the cervix uteri, from the want of adequate diagnostic marks to distinguish them; or that, though non-malignant in its commencement, the cauliflower excrescence may, like some other local benign growths, become the seat of carcinomatous deposit and malignant action during its progress. Thus it has been found by Gooch and Madame Boivin to return again in a malignant form, after its imperfect removal by the ligature or knife. In an instance mentioned by Dr. Davis, its removal was followed, after the lapse of a considerable period, by its reproduction, and ultimately by carcinomatous ulceration; and in two cases that occurred to Professors D'Outrepoint and Siebold, in which large tumours having a cauliflower form were found affixed to the cervix uteri during parturition, the neighbouring uterine tissues, as well as the contiguous structures of the bladder and uterus, were found in a carcinomatous state upon the post-mortem dissection. In another case, in which Michaelis excised what he terms a *fungus medullaris* with a cauliflower appearance, from the anterior lip of the uterus during labour, the posterior lip of the organ afterwards degenerated, and cancer of the stomach ultimately supervened."

The opinions expressed in the two preceding paragraphs respecting the pathological nature of the cauliflower excrescence are, we venture to think, somewhat incongruous, and as such are scarcely tenable in the present state of pathological science. We believe that Dr. Simpson is in error in regarding the disease in any of its stages as non-malignant, and are of opinion that all its varieties are essentially referable to two forms of cancer—the epithelial and the medullary. Probably it would be more correct to restrict the term to the former variety alone, to which in its *genuine* forms its anatomical affinities are closest. By doing so, the discrepancy in the views of Dr. Simpson would be reconciled, and the malignant and non-malignant forms or stages of the disease described by him would be found to be strictly accordant with the ordinary developments of this form of cancer. We may observe that Virchow has described three kinds of papillary tumours of the os uteri—the simple, the canceroid, and the medullary; but the two first—to which the term cauliflower excrescence should, we think, be restricted—have been shown by Mr. Paget to be but the different stages of the same disease—viz., epithelial cancer.

He remarks, with reference to the opinions of Virchow, that—

"It is evident from his description that the cauliflower excrescence, in the two conditions distinguished by him; illustrates the usual history of the most exuberant epithelial cancers; it might," he adds, "be taken as the principal example of the group. That which he calls the '*simple papillary tumour*,' is an excessive papillary outgrowth of epithelial cancer; the later stage of the same, when it passes into '*canceroid*,' is the usual extension of such a cancer into deeper parts—a continuous growth of the same thing in a new direction; for the papillary structures—composed, as Virchow says, of epithelial cells with bloodvessels and a very little connective tissue—are the essential characters of the epithelial cancerous outgrowths; and I believe that the same composition has never been seen in any papillary or warty growths that did not, if time were allowed, proceed to the formation of epithelial structures in the deeper parts, and thence through the usual progress of malignant disease."*

We believe that these views are essentially correct: that the cauliflower excrescence is but an epithelial cancer of the cervix uteri, and as such,

malignant in its nature throughout, although the more evident indications of such malignancy are only manifested in the later stages of its progress. By bearing in mind this fact, and the ordinary course of epithelial cancers, we are enabled to reconcile the very incongruous views respecting the nature of the disease which have been put forward by different writers. In its earliest stage it appears as a simple papillary tumour, without any sign of malignancy, and as such has suggested the opinion that it was of a simple, benign nature. In its further progress, however, those characters become superadded which leave no doubt of its malignancy, and thus, according as the disease is studied or observed in either of these stages, opinions have varied as to its pathological nature.

In discussing excision of the cervix uteri, Dr. Simpson states that the following forms of disease furnish cases for the operation :

1. Great morbid hypertrophy, by elongation of the vaginal portion of the cervix uteri.
2. Corroding ulcer, when limited to the lips of the cervix, and pathologically identical with the form of lupus or malignant ulcer so well known on the face; and,
3. Circumscribed and local forms of carcinomatous disease, or excrescence of the lips and lower segment of the cervix uteri.

We think this series is somewhat too extended. Cases of simple hypertrophy of the cervix would, in our opinion, very rarely require so severe an operation; and with regard to corroding ulcer of the uterus, it appears to us that its generally insidious and irregular progress would render it very seldom amenable to operative treatment. At the present time we have a case under our care, in which, before the patient was aware of having any serious uterine disease, the posterior segment of the cervix was found to be entirely eaten away, as well as a portion of the corresponding part of the body, whilst, at the same time, the anterior part of the cervix was still partially intact. We think, therefore, that the question of the excision of the cervix uteri is practically limited to cases of cancer, or rather, we would say, to one form of cancer—viz., the epithelial or cauliflower excrescence, and for the following reasons.

Regarding the three principal forms of cancer which are met with in the uterus—viz., the scirrhus, the epithelial, and the medullary—we apprehend that there are very cogent reasons, founded upon their pathological history, against the performance of the operation in cases of either the first or the last; and there can be no doubt that many of the unsuccessful cases, in which it is said to have been adopted for the removal of cauliflower excrescence, were really cases of medullary cancer. Restricting the term cauliflower excrescence, however, to epithelial cancer of the cervix, we would observe that there are many circumstances deducible from the general history of this form of cancer, which we think would justify the performance of the operation at an early stage of the disease. It is the peculiarity of this variety, to remain long in a local form, and to be attended with the fewest symptoms of constitutional contamination. In the words of Mr. Paget—

“Among all the cancers, the epithelial present the general or constitutional features of malignant disease in the least intense form. They commence at the

latest average period of life; they appear to be most dependent upon local conditions; they are least prone to multiplication in internal organs, and they are associated with the least evident diathesis or cachexia."*

We would submit that these circumstances are sufficient to justify our resorting to the operation in certain cases, and the success of Dr. Simpson in the one he reports, is calculated to give us additional encouragement.

A re-perusal of the very remarkable memoir *On Retroversion of the Unimpregnated Uterus*, has only served to convince us that, whether regarded with reference to its literary merits or practical tendency, it is one of the most specious and least satisfactory of all Dr. Simpson's obstetric contributions. The leading doctrines and practical deductions put forward in it may be summed up in the following propositions:

1. That the disease had generally been considered to be rare, merely in consequence of the want of a proper and easy means of detecting it; but that since the author had made this discovery, he had found it to be one of the most common and frequent displacements and affections of the unimpregnated uterus;

2. That the functional symptoms of this lesion are of a very varied and uncertain character, sometimes being either few or altogether absent, and sometimes extremely severe and distressing. That when present, they are more or less perfect imitations of the secondary phenomena of pregnancy, such as dyspeptic and hysterical symptoms, neuralgic pains in the mammæ, in portions of the vertebral column, or in the parietes of the chest or abdomen. Mechanical irritations and symptoms in neighbouring organs, more especially of the rectum and bladder, giving rise to constipation or impeded defæcation on the one hand, or to dysuria, retention, or incontinence on the other. Weight, tension, and bearing down in the regions of the uterus and rectum, with dragging at the loins and in the regions of the uterine ligaments. Pains stretching down one or both lower extremities. Inability to bear carriage exercise, whilst walking and standing speedily produce fatigue. In some cases menstruation is not morbidly altered, but in others it is either suppressed, painful, or excessive. Leucorrhœa is sometimes absent, and at other times present; and when pregnancy occurs, abortion is apt to take place; but in some cases the uterus is spontaneously rectified, and utero-gestation proceeds to the full time. Usually it interferes with the function of conception, and is often a cause of sterility;

3. That with regard to its physical signs, the employment of sight by means of the speculum in no respect assists our diagnosis; but that, on tactile examination, a solid tumour is to be felt behind the cervix uteri, between the uterus and rectum, and the same firm mass may be felt through the anterior wall of the bowel, in making an anal examination; and that the most simple and certain method of diagnosis consists in the introduction of the uterine sound, which, by following the curvature or reflexion of the uterus, can be made to enter and traverse the centre of the supposed tumour;

4. That the most frequent abnormal conditions with which it is apt to be confounded are—*a.* Pregnancy. *b.* Fibrous and other tumours in the

posterior wall of the uterus. c. Ovarian tumours in their earlier stages. d. Pelvic cellulitis. e. Extra-uterine conceptions lodged between the uterus and rectum. f. Organic disease in the anterior wall of the rectum. g. Stricture of the rectum. It is scarcely necessary to add, that the principal means relied upon for diagnosis is the introduction of the uterine sound.

5. That the treatment should comprise the three following indications: a. The removal, if necessary and possible, of any morbid action in the uterine structures that may exist along with the displacement. b. The restoration of the uterus to its normal situation. c. The use of means to retain it in its replaced and natural position. The first indication points to the removal of congestion, inflammation, and hypertrophy of the uterus—constriction of the cervix uteri; and painful, congestive, and inflammatory conditions of one or both ovaries. The second to the restoration of the uterus to its normal situation, which is best effected by introducing the uterine sound into the cavity of the organ, and using it as a lever. The retention of the replaced uterus in its normal situation is proposed to be accomplished by means of an intra-uterine pessary, which is to be worn for a period varying from one or two weeks to many months.

In reviewing the general character of this paper we are, in the first place, struck with the want of logical concatenation between the premises stated and the conclusions arrived at. We fail to see any necessary connexion established between the lesion in question and the many abnormalities with which it is said to be associated: or, in other words, there is a want of proof that it, *per se*, is the cause specifically of the several consequences which are imputed to it. We believe, in fact, that there is some misapprehension in the mind of our author as to the pathological importance and significance of this lesion; and are led to conclude, from clinical observations, that the uterus is altogether a passive agent, both in its production and the causation of the many morbid phenomena with which it is associated. We would observe that this organ has no fixed position in the pelvis, but is liable to be altered both in its position and axis by a variety of extraneous agencies—by the state of the urinary bladder in front, of the rectum behind, the intestines above, and that of the colon on either side; and consequently, from a variety of morbid states incidental to these organs, it is liable to be deposed from its natural position, and to have its axis altered from its normal state. If we mistake not, it will very commonly be found that the same causes which give rise to abnormal states of the chylopoietic or abdominal viscera, simultaneously disturb the functions of the uterine and pelvic. Now, if it be true that the uterine organs suffer in these cases in a secondary rather than in a primary manner—if it be true that the retroversion and coincident disturbance of the uterine functions follow upon and are the consequences of the more general derangement referred to, rather than its cause—then it must be obvious that any treatment, to be successful, must be directed to the original cause rather than to the secondary effect; or, in other words, to the various antecedent conditions out of which the uterine malady, together with the others, may be said to arise. In support of the correctness of these views we would observe that cases of

retroversion have come under our care in which there was considerable uterine and constitutional derangement, and in which the greatest benefit was derived from treatment exclusively addressed to the cure of the latter, notwithstanding that the retroversion remained. We have also met with others in which the displacement was discovered by means of the uterine sound, in which little or no uterine or constitutional disturbance was present. Now these circumstances are calculated to make us hesitate in accepting the theory which would impute to this lesion so many of the most distressing sufferings and ailments to which the female sex is obnoxious, as set forward in the text.

But if the pathological nature and relations of a lesion are doubtful, it becomes still more necessary to be cautious in the application of our therapeutical or curative measures; and here we cannot help thinking that the means suggested for its cure are scarcely warranted by our knowledge of the nature of the disease. It is proposed by Dr. Simpson, first, to replace the organ by means of the uterine sound, and secondly, to fix and retain it in position by means of an intra-uterine pessary—that is to say, by means of an inflexible and unyielding instrument, which can permit of little or no uterine movement. Now, apart from the difficulty and danger which are said to have attended this proceeding, we are prepared to oppose the practice upon purely physiological grounds. We have already adverted to the fact that the uterus was never intended to hold a fixed and undeviating position, but to accommodate itself somewhat to the varying states of adjacent organs. In the words of Dr. Simpson, its position is capable of being changed to a very considerable extent without incommo-^{dity} or injury, by such exterior influences as may naturally or accidentally act upon it. Its position is so far constantly changed by the varying states of distension of the bladder and rectum. Under voluntary efforts of straining it can in general be readily pushed down half an inch or an inch into the cavity of the vagina, and it may be drawn down by instruments till the cervix reaches the external parts themselves, or even protrudes beyond them. (pp. 50, 51.) Now we contend that to fix an organ which nature never intended to be fixed, and to do away with all those necessary movements by which the uterus is enabled to accommodate itself to the varying conditions and necessities of neighbouring organs, is not only to act in opposition to all sound physiological principles, but to adopt a practice which, apart from its dangers, must be productive of very doubtful advantages.

Four papers are contributed upon the subject of Ovarian Dropsy. In the first the object of the author is to recommend the horizontal, in preference to the erect, position of the patient for the operation of paracentesis, the advantages being that the necessity of a bandage is thus done away with, and the tendency to fainting and syncope, for the prevention of which it is especially recommended, is obviated; whilst at the same time it is remarked that the contents of the dropsical cyst or cysts are more easily and more completely evacuated than when the operation is performed while the patient is in the upright position.

In the second, Upon Inflammatory and Non-inflammatory Ruptures of Ovarian Dropsical Cysts, the following conclusions are arrived at:

"1. The cysts forming an ovarian dropsy occasionally rupture, first, from inflammatory effusion into, and distension of, their cavities; or secondly, the contents of the cysts being only the common bland secretion of such cysts, and unmixed with any inflammatory matter, they may rupture from mere over-dilatation and gradual attenuation of their coats, or under sudden mechanical pressure and injury.

"2. When a cyst ruptures from the effects of inflammation, or contains within it, at the time of rupture, inflammatory secretions and materials, the escaping fluid, if effused into the cavity of the peritoneum, is always liable to be followed by dangerous, and generally fatal, peritonitis.

"3. If, however, a cyst burst into the peritoneum under mechanical injury, or in consequence of simple laceration from over-distension of its cavity, and the fluid effused into the sac of the peritoneum is consequently not commixed with inflammatory secretion, there is little or no great tendency to peritonitis.

"4. Sometimes, indeed, when a non-inflamed ovarian cyst thus ruptures into the cavity of the peritoneum, the life of the patient is preserved, or at least prolonged, by this accident.

"5. When an ovarian cyst ruptures into a mucous canal, or upon the cutaneous surface, the safety or danger attendant on the laceration is not regulated by the inflamed or non-inflamed character of the effused fluid.

"6. In cases in which the fluid of an ovarian cyst obtains an outlet by a mucous canal, or by the skin, a temporary or more permanent reduction of the tumour, and comparative cure of it, may be the consequence.

"Lastly, let me add that, as in many cases and points the surgery of art is an imitation of the surgery of nature, possibly the artificial repetition and establishment of the above modes of relief, if they could be imitated safely and certainly, may yet be found capable of temporarily arresting, if not curing, ovarian dropsies in some appropriate cases, and more particularly in instances in which the great bulk of the tumour is formed by one original large preponderating cyst, or by several cysts broken up and conjoined into one common cavity or cell." (pp. 258, 259.)

In the third paper, On the Treatment of Ovarian Dropsy by Injections of Iodine into the Cysts, it is stated that the author had so treated some ten or twelve cases; that the disease had recurred in a few of these cases, but that in others the cure had as yet been permanent; that the operation was unattended with any bad results; that little or no uneasiness had been expressed by the patients during its performance; and that the common Edinburgh tincture of iodine had been employed, usually to the extent of two or three ounces.

In the fourth—On Ovariectomy—the question is discussed—Is it, or is it not, an operation justifiable upon the common principles of surgery? and the conclusion arrived at may be thus briefly stated: that it is an operation of a most serious and dangerous character, and quite unjustifiable in many of the cases in which it has been resorted to; that when the health and life of the patient are not immediately threatened by the stage and progress of the malady, when the tumour is a source of inconvenience and deformity rather than a source of danger, and when the evils of the disease are as yet prospective rather than real, it ought not to be attempted: but that if the health of the patient were becoming rapidly undermined by the disease; if the progress of the affection showed that, ere long, it would inevitably prove fatal; if the question were reduced to one of certain and not distant death, from the course of the malady, or possibly an entire escape from the affection, with prolonged life and health from the operation; and if, in addition, it were ascertained that the tumour

was free from adhesions; and other circumstances were such as to present no counter indication—then, and then only, should it be undertaken.

In the general tenour of these remarks we fully concur; and we can only hope that the admitted danger and fatality of the operation, even by those who are not altogether opposed to its performance, may direct professional attention to the discovery of other means of treating this affection than purely operative proceedings; for it is scarcely too much to affirm that, for the last fifteen or twenty years, the treatment of the disease has mainly consisted of a succession of surgical experiments, each in turn lauded as successful, and each in turn abandoned for some other. We may enumerate, for instance, besides tapping—pressure; tapping and pressure; tapping and injection of iodine into the cyst; artificial openings in the cyst made to communicate with either the surface, the vagina, or the rectum; excision of a portion, large or small, of the cyst; incision; extirpation and ligature; and yet it is not too much to assert that, after all that has been written and done, our practice is not more successful, our knowledge of the pathology of the disease is not more advanced, nor the tenure of life in the case of persons suffering from it more secure, than in the days of our more empirical forefathers. Nor can any one, probably, claim a greater success in the treatment of the disease at the present time than that claimed by the late Dr. Hamilton, who stated that, after sixteen years' trial, he had succeeded, in a number of cases, in curing or retarding it by the simple means of firm compression of the abdomen, percussion, the use of the warm bath, and a protracted course of the muriate of lime, together with the ordinary means for promoting general health.

What we require as a first step to successful practice is a knowledge of the circumstances under which these cystic growths arise; for that their origin is determined by the operation of some abnormal stimulus acting upon the uterine system is a proposition the truth of which is too evident to be doubted. Being immediately dependent upon the abnormal evolution of one or more Graafian vesicles, we can at once trace their connexion with some form of menstrual irregularity; and from a careful study of the various causes of menstrual disorders, we may ultimately hope to educe the principles upon which their development may be arrested or prevented. We have reason to believe that such an inquiry will prove more promising and successful than might, at first sight appear, but it is one necessarily of a very comprehensive character. It should comprise an inquiry into all the various causes, local and constitutional, by which the uterine organs may be unfavourably impressed, including direct and secondary causes of irritation or excitement, the study of various constitutional taints and diatheses, hereditary influences, and that of various abnormal conditions of the blood and nervous system; for we are persuaded that the causes of the disease vary greatly in different cases, and comprise an extensive range, from mere simple local irritation or excitement, to that grave form of constitutional derangement out of which the colloid and semi-cancerous forms of the disease arise. It must thus be evident, that the clue to successful practice lies in a right apprehension of the particular nature of the cause in each case; and we are assured that, by investigating and correcting this in individual instances, we have been enabled to arrest their progress in some; and to effect their resolution in others. We know

that, under varying circumstances affecting the constitution, these growths have entirely disappeared—the fact is certain, although it is at present inexplicable, but it serves to show that their origin and increase have a fixed and specific relation to certain morbid or peculiar states or actions of the economy, and that, if these could be accurately discerned, we might successfully co-operate with the natural efforts in effecting their spontaneous cure. We have briefly indicated the preceding views in connexion with the subject before us, but are compelled, from a regard to space, to forego entering more fully into them for the present.

We conclude our notice of the papers in the first part of the volume by a brief reference to two, On the Pathology and Treatment of Dysmenorrhœa. In the first, the author combats the prevalent opinion, that the membranous structure occasionally expelled in some forms of the disease is a mere exudation of coagulable lymph, occasioned by inflammation; and contends, and we think successfully, that it consists, in reality, of the superficial layer of the mucous membrane of the uterus itself, hypertrophied and separated. He enters at length into a consideration of the several grounds upon which this opinion is founded; but we are unable, from want of room, to give them in detail. In the second, the operation of division of the cervix is recommended for the cure of obstructive dysmenorrhœa, and the nature of the proceeding, as well as the instrument employed by him for the purpose, is described at some length. We are not in a position to judge of the merits of the operation, having neither performed it ourselves, nor witnessed the effects of its performance by others; but we are led to believe that the necessity for the practice might be obviated, at least in some cases, by the free exhibition of alkalies before and during each menstrual period. We have known such practice useful in cases in which there was an unusual constriction of the cervical orifice and canal, and in which the menstrual suffering appeared to be referable to this cause. We believe, moreover, that this practice is supported by various physiological considerations. The administration of alkalies tends remarkably to increase the fluidity of the blood, and this fact was strikingly brought under our notice some years ago, in a case in which they had been largely administered, with the view of testing their value in the treatment of consumption, given for the purpose of promoting the absorption of tubercular deposits. Now, as in some forms of dysmenorrhœa there appears to be a want of relation between the tenuity of the uterine secretion and the passage by which it has to escape, as this in many cases is clearly traceable to a rheumatic diathesis by which the fibrinous constituent of the blood is increased, and as from this condition we may deduce not only the abnormal stimulus by which the lining membrane of the uterus is excited to an abnormal action, but also the increased plasticity of the menstrual secretion which results, we think we may venture to affirm that the practice has something more than mere casual observation in its favour. Finally, we may observe, with reference to the supposed dependence of sterility upon constriction of the cervix, that we have at present the case of a lady under our care, in which pregnancy has supervened for the first time, many years after marriage, apparently as the result of general treatment specifically directed to the invigoration of the uterine organs; although, some months ago, it had been

announced, by an eminent accoucheur, to be physically impossible, from the small size and great constriction of the cervix and os uteri.

The papers On the Physiology and Pathology of Pregnancy need not detain us long. The most important of the series, which is limited in number to five, is one On the Duration of Pregnancy, in which the author argues, from various facts observed in the human female and the lower animals, more especially the cow, that it is not so fixed or determinate as is commonly supposed, and that it may be prolonged from thirty to thirty-five days beyond the 280th, the limit usually assigned to it.

The third and concluding part of the volume contains several important papers On Natural and Morbid Parturition, which we will briefly consider, as they respectively relate to the subject of natural, tedious, instrumental, preternatural, and complex labour. In connexion with the first of these varieties, we may refer to some interesting observations on the mechanism of natural labour, and the mode in which the foetal head enters the brim of the pelvis. It would appear that at different times very different views have prevailed upon this subject, but the more exact and extended investigations of Professor Naegelé, which are supported by those of the author and Dr. Martin Barry, tend to show that it almost invariably enters in the direction of one of the oblique diameters, and, with rare exception, with the vertex directed either to the left foramen ovale, or to the right sacro-iliac synchondrosis. The former is the most frequent of the two; but it is to be observed, that in the great majority of cases in which the vertex is primarily directed to the right sacro-iliac synchondrosis, it gradually rotates forwards, and emerges under the arch of the pubis, as in cases in which it originally presented at the left foramen ovale. The importance of attending to this circumstance in the application and use of the forceps must be too obvious to be insisted upon.

Of the various papers relating to the subject of tedious labour, we would more especially direct attention to those, On the Sex of the Child as a Cause of Difficulty and Danger in Human Parturition, On Irregularities of Head Presentations, the Treatment of Face Presentations, and On the Influence of Galvanism on the Action of the Uterus. In the first of these, the author adduces many facts to show that the danger to the mother and child is greatly increased in the birth of male children, and that the cause of this danger is the comparatively larger size of the head of the male than the female infant. In the papers On Irregularities of Head Presentations, and the Treatment of Face Presentations, he very judiciously enforces the principle of non-interference; advising merely the employment of such means as may act upon the mother, so as to render either the uterine action more decisive, or the parturient passages more dilated, or at least more easily dilatable. The paper, On the Influence of Galvanism on the Action of the Uterus, although very elaborate, is, in our opinion, far from being conclusive, and is opposed to the results of our own experience, as well as that of many other practitioners. Dr. Simpson's observations have led him to the conclusion that galvanism exercises no influence upon the uterus during labour, and he even intimates an opinion, that the uterine structure, like that of the dartos, is incapable of being excited by it. We have no room to discuss the question

at length; but feeling that the results of clinical observation were open to several sources of fallacy, we determined to test this question in a more direct manner, by exposing the gravid uterus of a pregnant bitch, and directly observing the effects of galvanism upon it. The experiment was made at University College, in the presence of Dr. Boon Hayes, the teacher of Practical Anatomy and Physiology in that institution; and Mr. Statham, Assistant-Surgeon to University College Hospital, and the following were the results: In rather less than ten minutes after the two poles of a common electro-galvanic machine of moderate intensity had been applied to the uterus, a slight vermicular action of the enclosed portion of the organ was perceivable. In rather more than a quarter of an hour, the uterus became distinctly firm and tense during the period of the contact of the two poles, and it again relaxed on their removal—the tension of the parietes returning on re-applying the poles, and subsiding on their withdrawal; in rather more than half an hour from the first application of galvanism, labour had fully supervened, as indicated by the peculiar cries of the animal, the firm contraction of the uterus, and the propulsion of one of the fetuses towards the vagina. It should further be observed, that uterine contraction was more powerfully produced when one pole was applied to the upper part of the spine and the other to the uterus, than when both were applied to the latter organ. We will not comment further upon these facts than to observe that they appear to be decisive, on the one hand, as to the influence of galvanism upon the gravid uterus, and on the other, of its greater influence when applied through the medium of the spinal cord and nerves, than to the uterus itself.

On the subject of instrumental labour the reader will find some useful observations on the mode of application of the long forceps, an account of the air tractor as a substitute for the forceps in tedious labours, and a very elaborate memoir, On Turning as an Alternative for Craniotomy and the Long Forceps in Deformity of the Brim of the Pelvis. The theory of the proposed practice is mainly based upon a consideration of the obstetric configuration of the foetal head, and the comparatively smaller measurement of the bi-mastoid than the bi-parietal diameter. We fear, however, that the difficulty attending the selection of proper cases for the practice, and the increased danger to mother and child should the operation miscarry, will cause it to be rarely resorted to in this class of cases.

In treating of preternatural labour, the author lays down the following rules with regard to the operation of turning—1st. That it is not advisable to seize both feet of the infant, as some writers have recommended, except in cases in which the uterus is ruptured, or in those in which one lower extremity having been brought down, the other is firmly embraced by the os uteri. 2ndly. That it is preferable to seize the knee rather than the foot. 3rdly. That that extremity should be selected which is on the opposite side of the body to that which is presenting; and 4thly. That the practice of rotating the child when the toes are directed forwards is, for the most part, unnecessary, inasmuch as in the majority of cases there is a spontaneous tendency to this rotation as the birth of the child proceeds without any manual interference.

By far the most important contribution on the subject of complex labour is the well known memoir, On the Spontaneous Expulsion and Artificial Extraction of the Placenta before the Child, in Placental Presentations, wherein the author enforces this practice in certain cases of unavoidable hæmorrhage in which the other recognised modes of management are either insufficient or unsafe, or altogether impossible of application. He founds his justification of the practice mainly upon two considerations; the first relating to certain physiological views respecting the placental origin of these hæmorrhages; the second to the fact that in several cases the placenta has been spontaneously separated and thrown off from the uterus before the birth of the child, not only without any increase of hæmorrhage, but with either its diminution or entire cessation. With regard to the first of these considerations we have already laid before the profession the grounds upon which we are led to conclude that the chief source of hæmorrhage in these cases is not the detached portion of the placenta, as assumed by Dr. Simpson, but rather the torn utero-placental arterics which lie on the denuded surface of the uterus. We are, therefore, constrained to believe that the sole ground upon which the practice can be sanctioned is the fact that in certain cases the placenta has been spontaneously separated and expelled before the birth of the child, with a marked diminution or cessation of the previous hæmorrhage; whilst in others the same result has been attained by its artificial separation and extraction. On these grounds alone it appears to us the practice can be justified—never, indeed, as the rule, but always as the exception.

The number and variety of the papers contained in the present volume have obliged us to omit the notice of many of great value, and to touch upon others with greater brevity than their importance would otherwise demand. We have, as stated at the outset, confined ourselves to an examination of those which more particularly represent the peculiar views and practice of our author; and in venturing to differ from him on certain points, we trust that we have stated the grounds of our dissent with that deference which is due to a writer to whose labours the profession and society are so largely indebted.

F. W. Mackenzie.

• REVIEW XI. •

1. *Elements of Chemistry, Theoretical and Practical.* By WILLIAM ALLEN MILLER, M.D., F.R.S., F.C.S., Professor of Chemistry in King's College, London. Part I., *Chemical Physics*.—London, 1855. 8vo, pp. 428.
2. *A System of Instruction in Qualitative Chemical Analysis.* By Dr. C. R. FRESENIUS, Professor of Chemistry and Natural Philosophy at Wiesbaden. Fourth Edition.—London, 1855.

A TEXT-BOOK intended as a guide in the pursuit of any of the branches of natural science stands in need of almost constant revision; otherwise the rapid progress of science will soon detract greatly from its utility. And if this remark be more or less applicable to every branch of natural science, how much more evident does it appear when applied to chemistry

and chemical physics, in which fresh discoveries succeed each other with such amazing rapidity, almost from year to year? We therefore hail with unmixed satisfaction the publication of the first part of Professor Miller's new book on chemistry, entitled 'Chemical Physics.' It is devoted to a subject upon which, as far as we know, no elementary work has appeared in this country since the publication in 1843 of Professor Daniell's 'Introduction to the Study of Chemical Philosophy;' a treatise which, however complete for the period at which it was published, is, we believe, out of print, and besides could hardly have been adapted, without extensive changes, to the teaching of the present day. Dr. Miller has, we think, judged rightly in bringing out an entirely new work on the subject; and, although he modestly describes it as having been originally intended as a text-book for the use of the students of King's College, we do not hesitate to assert that the result of his labours will be found useful not only to the pupils of King's College, but also to persons far beyond the circle of his own immediate class.

Professor Miller's book 'On Chemical Physics' constitutes an excellent, and in our opinion an indispensable introduction to the study of chemistry. That vast science has of late been so much extended as to require for its prosecution not only a superficial knowledge of, but an intimate acquaintance with most of the important laws of experimental physics. Chemistry, in fact, takes cognisance of all changes in the constitution of matter, whether these changes be effected by purely chemical agencies, or whether they be the result of heat, electricity, or other physical means. The mode of action of these different forces must, therefore, necessarily be rendered familiar to the student before he can attempt to become acquainted with the chemical results to which they may give rise.

The first part of Dr. Miller's work may be considered not only as an indispensable introduction to chemistry, but also as an important store of medical knowledge. There exists a peculiar class of physiological phenomena, which the distinguished physiologist, Magendie, has called *physical phenomena of life*, and Matteucci, *physical phenomena of living bodies*; consisting of certain physical functions of the animal body, as absorption and circulation (which include capillary attraction, gravitation, &c.), as the physical effects of animal heat upon the body, animal electricity, &c., &c. The study of these physical phenomena of life, except perhaps that of animal electricity, has been singularly neglected of late, principally because natural philosophy is erroneously considered as not connected with medicine. We beg, therefore, to impress upon our medical readers the importance of their being well acquainted with the physical laws of organic nature, with the view of applying this science to the pathology and treatment of certain abnormal states of the physical functions of the human body; and though Dr. Miller's work be not written exclusively with the view of imparting this knowledge, still it will afford the reader much useful information, to be applied to the above-mentioned branch of physiology and pathology. The second volume of this treatise, which will be devoted exclusively to Inorganic Chemistry, is expected to be ready by the end of the present year; and the third part, including Organic Chemistry, is advertised for the spring of 1856.

The volume which we have at present before us is divided into six

different chapters—we would rather have called them “parts.” The first chapter, devoted to a preliminary view of the nature and characters of chemical affinity, and to the laws of chemical combination, affords the author an opportunity of drawing a distinct line between the physical and chemical properties of bodies; though, as he himself observes, the limit between the two sciences is of no great importance, since the chemical nature of any substance can be but imperfectly studied without a tolerably complete knowledge of its leading physical characters. The characters and laws of chemical affinity, the different modes in which chemical compounds are formed,—namely, by direct combination of two substances one with the other, or, as is more commonly the case, by the displacement of one of the ingredients of a body by another substance, and the formation of a new compound,—are next successively treated. The laws of “definite and equivalent proportions,” with their application to the construction of “tables of equivalent numbers,” and the principles of symbolic notation, are rendered as intelligible as possible to beginners; but the difficulty of illustrating these laws by familiar examples to students who are supposed to be totally unacquainted with chemistry, makes us somewhat regret that the author did not defer this portion of his subject to a later period, when it might have been expounded with less difficulty and with a greater certainty of being fully comprehended by those to whom it is addressed.

After a short explanation (in chapter second) of the different systems of weights and measures, the author proceeds, in the third chapter, to examine at considerable length the various modes in which the forces called *molecular*—that is, acting between the particles of bodies at inappreciable distances only—are supposed to operate. He classes them under the following heads: *Elasticity, Cohesion, Adhesion, and Crystallization.* The study of the first, elasticity, which ought, perhaps, rather to have followed than preceded cohesion and adhesion, furnishes him an opportunity of explaining the mechanical properties of gases, and describing the principal instruments depending upon the pressure and elasticity of the atmosphere. In adverting to adhesion, or the power which holds together particles of dissimilar kinds of matter, the author remarks on the importance of this property for the chemist, as being more nearly allied than any other force to chemical affinity. Adhesion, in fact, gives rise to a variety of important phenomena; it is the principal agent employed in the production of capillary action, of solution, of the diffusion of liquids, of endosmosis, and less directly in the process of the intermixture and diffusion of gases. Our author examines its action in each of these different phenomena, illustrating each case by useful or curious applications, whenever an opportunity offers. In treating of the adhesion between liquids, Professor Graham's late curious investigations on the diffusion or gradual intermixture of liquids of different densities, are minutely described. Intimately connected with the process of liquid diffusion, are the changes known under the name of endosmosis and exosmosis, which occur when the two liquids are separated from each other by a porous diaphragm, and in which, as Dutrochet first observed, the process of mixture goes on in opposition to the direct action of gravitation. Dr. Miller, after having observed that these phenomena have lately acquired additional importance

in a chemical point of view, proceeds to describe the principal results obtained by means of Professor Graham's *osmometer*, and shows that in almost all cases of osmose or osmotic action, a chemical action on the material of the diaphragm, whether it consist of bladder or of earthenware, invariably occurs—indeed, appears in some degree inseparable from the existence of the osmotic phenomena, as a diaphragm composed of porous materials not susceptible of decomposition by the liquids, gives rise to little or no osmotic action. This circumstance is especially interesting in a chemical point of view, as is the following, which we also owe to Professor Graham:—that two salts, such as sulphate and carbonate of potash, for instance, when mixed together, often have an osmotic action very different from that which they exercise separately. Again, chloride of sodium, when added, even in the most minute quantity, possesses the power of reducing in a remarkable degree osmotic action in other salts.

The diffusion, or process of intermixture, of gases, is next examined. Our author arranges the dynamic conditions of gases under four heads: *Diffusion*, or intermixture of one gas with another; *Effusion*, or escape of a gas through a minute aperture; *Transpiration*, or passage of gases through long capillary tubes into a rarefied atmosphere; and *Endosmosis*, or passage of gases through intervening diaphragms. Professor Graham's curious experiments on this interesting subject, more particularly those on the rate of effusion and transpiration, are minutely described, and several new facts are adduced, showing the importance of the subject in a practical point of view, and the various applications of which it is susceptible. We cannot do better in this instance than cite our author's own words:

“The process of diffusion,” he says, “is one which is continually performing an important part in the atmosphere around us. Accumulations of gases, which are unfit for the support of animal or vegetable life, are by its means silently and speedily dispersed, and this process thereby contributes largely to maintain that uniformity in the composition of the aerial ocean which is so essential to the comfort and health of the animal creation. Respiration itself, but for the process of diffusion, would fail of its appointed end, in rapidly renewing to the lungs a fresh supply of air in place of that which has been rendered unfit for the support of life by the chemical changes which it has undergone.” (pp. 83, 84.)

The fourth chapter of Dr. Miller's work, “upon Light,” may be considered as constituting an elementary treatise on all those portions of the science of optics which bear directly or indirectly on chemistry. Till within the last few years, the subject of optics would have been thought to have but very little connexion with chemical phenomena; and indeed, as far as we are aware, this is the first time that a detailed account of the properties of light has been introduced into an elementary treatise on chemistry. Dr. Miller is, however, quite right; as he justly remarks, an acquaintance with the fundamental laws and properties of light has become of late indispensable to the chemist, pharmacist, and physician. A very few examples will suffice to test the truth of this assertion. In how many cases, for instance, does not the difference in the refractive power of the essential oils afford a good test of their quality and genuineness? Again, does not the action of polarized light furnish us a useful test for distinguishing the diamond and other precious

stones from spurious imitations? The varieties that are remarked in the amount and direction of circular polarization, are well known to indicate, with a tolerable degree of accuracy, the varieties and proportions of sugar to be found in complex saccharine liquids; and the polariscope of Soleil, founded upon this principle, and intended to detect the quantity of sugar to be found in the urine of diabetic patients, is, or ought to be, familiar to all medical practitioners. We need not insist on the impossibility of rendering these facts intelligible to students totally unacquainted with the laws of optics. Some correct ideas on the different theories of light, on the laws of reflection and refraction, and on the different modes of ascertaining the refractive power, some elementary but accurate notions on the theories of interference, double refraction, and polarization, are evidently an indispensable preparation to the study of the chemical effects of light. And we may add, that these fundamental principles are treated in Dr. Miller's work with so much judgment and lucidity, that no chemical or medical student ought to be deterred from a careful study of this chapter, through an erroneous idea that the subject may not be completely indispensable to the practical chemist, or that it is too difficult to be mastered without the aid of mathematics.

The two last, and we may perhaps add, most important, subjects treated of in the present volume, relate to Heat and Electricity. No explanation is required to convince our readers that without the knowledge of these two essential branches of physics, all attempts to master the science of chemistry would, and must, prove utterly abortive. In regard to heat, as our author himself observes, "There is scarcely a chemical operation in which heat is not either emitted, absorbed, or purposely applied to produce the required result;" and with respect to electricity, three quarters of a century have now elapsed since the discovery by Volta of that most powerful of all chemical agents, the voltaic pile, which in the hands of Davy led to the discovery of the metallic nature of the alkalis and earths, effecting by the decomposition of these substances a complete change in the aspect of chemical science. Indeed, the absolute necessity of a thorough acquaintance with all the leading principles of heat and electricity as an introduction to chemistry, has been so long and so universally acknowledged, that although by far the greater number of the modern treatises on chemistry contain little or nothing on many of the important subjects to which Dr. Miller has thought proper to introduce the student of the present day, heat and electricity, at least, although not treated with so much method and detail as in the present work, have never been completely passed over in the numerous elements and manuals of chemistry, published both in this country and on the Continent, since the beginning of the nineteenth century.

Dr. Miller, after a brief review of the different sources from which artificial heat may be procured, divides the vast subject of heat into three distinct paragraphs or sections: the first comprehending the phenomena of *expansion*, with their applications to the measure of temperature; the second refers to the different modes in which the equilibrium of temperature is sustained or restored—namely, *conduction*, *convection* or circulation, and *radiation*; and the third relating to heat in *combination*, including *specific* and *latent* heat, with their different applications to the

processes of congelation and liquefaction, as well as to those of ebullition and evaporation. In going over these different subjects, it is impossible not to be struck with the clearness and method which prevail throughout, and still more, perhaps, by the judgment and tact with which the author, although he leaves no important point untouched, avoids entering into useless discussions on delicate and contested questions, which would only embarrass the student, without affording him any additional facility for the prosecution of chemistry.

In the section relating to the equilibrium of temperature, our author, after having explained the different modes by which all bodies tend, either by conduction or radiation, to return sooner or later to the temperature of surrounding objects, adds a third mode, generally included in that of conduction, to which he gives the name of *convection* of heat. He understands by convection the well-known property by which liquids and gases, particularly the latter, deprived, as they are, almost entirely of the power of conducting heat, admit nevertheless of being rapidly heated or cooled, by a process of circulation producing currents, and depending on the extreme mobility of the particles that compose them. The history of the currents produced in gases by the expansive action of heat, affords the author an opportunity of entering into some interesting details in regard to their application to the ventilation of our dwellings, and also to the interesting phenomenon of the trade winds; which, as is well known, originate on a large scale in the processes of circulation produced by heat to which we have just alluded. The latter part of this interesting section is devoted to the experiments of Pictet, Leslie, Dulong and Petit, on the radiation, reflection, and absorption of heat; and finally to the still more delicate and important researches of Melloni, on the transmission of radiant heat through screens of different substances, and the singular consequences to which they have led.

The fourth and last section on heat treats of heat of *composition*, or, as it may be termed, heat of *combination*. The heat of composition exists either under the form of *specific* heat, and capable under this form of contributing to produce the ordinary phenomena of heat, such as expansion and increase of temperature in surrounding bodies; or else under the form of *latent* heat, when it disappears in large quantities, exhausted, as it were, by its own efforts, at the moment of producing the liquefaction of solid bodies, or the conversion of liquids into elastic vapour. Our author is naturally led to describe the different phenomena that accompany ebullition, and to mention some curious recently-discovered facts relative to the marked effect produced by the adhesion of the liquid to the surface of the vessel, in raising the boiling point many degrees above the usual standard. Professor Marcet, of Geneva, to whom the above experiments are chiefly due, ascribes to a similar cause the slight differences he has observed in the rapidity with which fluids evaporate, according to the nature of the vessel in which they are contained. The principal methods imagined for measuring the specific heat of bodies are next briefly described; the author, as might be expected, enters into a more detailed account of the phenomena appertaining to latent heat, and more particularly of those relating to the measurement of the latent heat of different vapours. This opportunity is not neglected

of making the chemical student acquainted with the theory of distillation, and the different apparatus generally employed for this purpose. A detailed account of the process used for reducing gases to the liquid, and even to the solid state, by means of pressure and intense cold, followed by a short description of Boutigny's curious experiments on the *spheroidal* state of liquids, concludes this important section, and with it the long and interesting chapter on heat.

The sixth and last chapter of Dr. Miller's volume contains an interesting and elaborate treatise on electricity and magnetism,—forces which are now so intimately connected that it is hardly possible to study the operations of either separately. This chapter, which, as the reader may at once perceive, embraces an immense variety of matter, is divided into eight distinct sections. After some clear though elementary notions on magnetism, and on the leading characters of magnetic action, the author examines successively, in the second and third section, the phenomena of *static* and of *dynamic* or *voltic* electricity. Under the head "Static Electricity" are treated the general phenomena presented by electricity in a state of rest, or, as it has been called, *tension*; such as those of attraction and repulsion, the laws of induction, the distribution of the electric charge, and also a few remarks on the two hypotheses by which these phenomena have been accounted for. The electric machines in general use, with their various applications, are next brought before the reader. A short account of the different sources of static electricity affords the author an opportunity for entering into some detail on atmospheric electricity, which, although its origin is still, to a certain extent, shrouded in mystery, may certainly be considered as the most important source of all. We regret that, in mentioning the phenomenon of the aurora borealis, and alluding to its electrical origin, the author has given no account of the remarkable explanation offered by Professor De la Rive, in which the aurora is ascribed to a series of electric discharges occurring in the Polar regions between the positive electricity of the atmosphere and the negative fluid of the earth; the previous separation of the two fluids being attributed to the unequal temperature produced in the atmosphere at different heights by the action of the solar rays, more especially in the Equatorial regions. This hypothesis has been admitted, we believe, by many philosophers, to offer a satisfactory explanation of most of the important facts connected with this singular phenomenon, more particularly of the connexion long since observed to exist between the aurora and the magnetism of the earth, as indicated by corresponding disturbances in the magnetic needle.* In section third, the author, after having described the origin of the voltaic pile, and the mode of measuring the intensity of the electric current by means of the galvanometer, enters into an examination of the conditions required for producing voltaic action. This affords him an opportunity of proving that, although contact of dissimilar substances may be necessary for the development of the electric current, no current is, in fact, produced without chemical action having previously occurred. The fact that the energy of the current has been shown to be proportionate to the intensity of the chemical action, and its direction dependent on the

* See *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève*, vol. xii.; and *Bibliothèque Universelle*, for 1853, vol. xxiv.

direction of this same action, is another satisfactory proof that chemical action is *essential* to the production of an electric current, and that Volta, in regarding the interposed liquid in his pile in the light merely of an imperfect conductor which allowed induction to take place through it, the electrical equilibrium being constantly disturbed by the contact of the two metals, totally overlooked the chemical changes which the liquid was constantly undergoing.

After these remarks on the theory of the voltaic pile, the reader is made acquainted with its recent modifications by Daniell, Grove, and Smee. The retardation or resistance experienced by the current from the very conductors by which its influence is transmitted is next examined, first in the case of a simple circuit, or when a single pair only of metals is employed, and afterwards in the case of a compound circuit. This affords the author an opportunity of describing Wheatstone's *rheostat* and *resistance* coils, an apparatus by which measured amounts of resistance may be introduced into the voltaic current; and also of giving an outline of Ohm's theory, in which the mutual action of the electro-motive forces, and the resistance of any current, simple or compound, are simplified by being represented under the form of a fraction.

The processes of voltaic discharge—first, by *conduction*, as when the circuit is completed by a wire, or any other good solid conductor; secondly, by *disruption*, when the current is greater than the conductor is able to convey, and when, as in the beautiful experiment with the points of charcoal, a luminous appearance is exhibited through a short interval of non-conducting matter; and, finally, the discharge by *convection* which takes place in liquids, and is accompanied by chemical action and transfer of the particles of the conductor, are next successively examined. The process termed *electrolysis*, by which compound bodies are resolved into their constituents by the action of electricity, naturally possessing peculiar interest for the chemist, belongs to this latter mode of discharge. Instead of being confined, as was formerly the case, to a certain number of insulated facts, this process is now shown to be dependent on certain definite laws, which the author passes successively in review. The presumed ignorance of the student in chemistry prevents his entering into many interesting details relative to the chemical effects of the voltaic battery, which are, no doubt, reserved for a future period.

Ørsted's celebrated experiment, in which the influence of an electric current upon a freely-suspended magnetic needle was pointed out for the first time in the year 1820, showing at once the close connexion which existed between magnetism and electricity, soon gave rise to a completely new science, known under the name of *electro-magnetism*. The general principles of this science, with their application to the various forms of galvanometers; the theory and mode of formation of electro-magnets, followed by the laws of electro-magnetism, due principally to Lentz and Jacobi, are successively treated in the fourth section of the present chapter. A clear and correct outline of Ampère's celebrated theory of electro-magnetism concludes this portion of the subject—which theory, as our author justly observes, "has satisfied hitherto the rigorous requirements of mathematical analysis, and explained all the phenomena of electro-magnetism that have been as yet discovered."

The limits of this article prevent us from referring at any length to the intricate but highly interesting subject of *volta-electric* induction, or production of secondary currents, obtained by inductive action from wires conveying currents in their vicinity, and that of *magneto-electric* induction, in which a current of electricity may be obtained in a closed conducting wire from the magnet. We can only say that, in our opinion, Dr. Miller has treated this complicated subject with a degree of method and lucidity which do him great credit.

The last section of this long chapter on electricity contains a summary of the late discoveries of Faraday and others on the relations of light and magnetism, and on the magnetism of bodies in general. The influence of magnetism on polarized light transmitted through certain uncrystallized transparent bodies, with the property these bodies possess, when placed between the poles of an electro-magnet, of rotating the polarized ray in different degrees and in different directions, according to the direction of the magnetic current, are succinctly but clearly described.

The author concludes by a more detailed account of Faraday's experiments on the magnetism of bodies in general, the distinction of bodies into magnetic and dia-magnetic, according to the position they assume with respect to the poles of a powerful electro-magnet; and finally, the influence which chemical composition and crystalline structure appear both to exercise upon the magnetic or dia-magnetic state.

We have now given the reader an insight into Dr. Miller's useful book, which, it will be perceived, is more particularly devoted to the description of the properties of matter in the statical condition; whilst a treatise on analytical chemistry, as the last edition of the valuable work of Fresenius, 'On Qualitative Chemical Analysis,' which we beg to bring before the notice of our reader, is especially calculated to convey a knowledge of the properties of matter in the dynamical state—or in other words, to give an account of the combinations and decompositions which organic and inorganic bodies may be subjected to, with the methods employed to induce these phenomena.

The increasing importance of analytical chemistry, both from its connexions with arts and medicine, has caused a fourth edition of the treatise of Fresenius, *On Qualitative Analysis*, to be published, and we are much indebted to Mr. Lloyd Bullock for having edited this valuable book. The last edition of this work will be found particularly useful to the medical profession, from its containing the methods to be employed for the qualitative analysis of mineral waters, and common water, and for the detection of poisons. Arts and agriculture will also derive much benefit from this useful publication. To the former, the knowledge of analytical chemistry will prove of the greatest value, as affording him the means of becoming thoroughly acquainted with the nature of the soil he is cultivating, and of the manure he must employ; and to him not only may the ground yield food in abundance, but also concealed treasures, as coal, metals, or even certain substances, as phosphate of lime, which, in the form of manure, are invaluable gifts to the agriculturist. The applications of chemistry are becoming daily more extensive, and many physicians and medical students will undoubtedly consult the work of Fresenius. The connexions of practical chemistry with medicine may be considered under three heads—

1. Chemistry applied to Pharmacy.
2. Chemistry applied to Toxicology, or the Detection of Poisons.
3. Chemistry applied to Physiology and Pathology, or the Study of the Immediate Principles of the Human Body.

The author has given in this volume practical directions, which will be found extremely useful for the study of pharmacy and for the detection of mineral and vegetable poisons—as arsenic, antimony, lead, copper, and morphia, strychnia, nicotine, &c. He has admirably described the operations necessary for the detection of these substances in the contents of the stomach, and other parts of the body where they are likely to be found; and from his practical experience in this, as well as in the other branches of analytical chemistry, the last edition of his treatise becomes a most valuable addition to medical literature.

We must here express our satisfaction at the author having omitted to allude to physiological and pathological chemistry. This is a science which must be cultivated exclusively by the medical profession; for as chemical analysis consists of the methods employed to decompose chemical compounds into their primary elements, physiological and pathological chemistry teach the methods of separating *mechanically*, by means of water, alcohol, and ether, the various constituents or immediate principles of animal tissues and fluids; which immediate principles are mixed together and held in their relative positions by the vital force. We are, however, so little acquainted with the methods of conducting this mechanical separation, that it is found necessary in many cases to have recourse to pure chemical analysis; the medical student must therefore make himself acquainted with the latter branch of chemistry, and he will find Dr. Fresenius's book extremely useful, both from the simple and accurate account of the manipulations he describes, and from the admirable classification of the various stages of analysis.

The beginning of the work is devoted to the principal operations in qualitative analysis—as solution, crystallization, precipitation, filtration, &c. The subject of crystallization, one of great interest, and of especial use to the medical student, might have been dwelt upon at greater length; and we regret that the author has not alluded more minutely to the methods employed for obtaining substances in the crystallized form.

He proceeds afterwards to describe the apparatus and utensils required for chemical analysis, and then gives a correct and detailed account of the reagents in the humid and dry way. Water, alcohol, and ether are mentioned as reagents; but the author observes, that in very few cases indeed do they serve to induce chemical decompositions, so that these fluids are rather to be considered as dissolving agents, more especially adapted to produce mechanical separations than chemical actions. It is for this purpose that water, alcohol, and ether are employed so extensively in physiological chemistry.

Reagents are divided into two classes—1. Reagents acting in the humid way. 2. Reagents acting in the dry way. The reagents in the humid way are subdivided into—(1) General reagents, or those principally used as simple solvents—as chemical solvents, and also those serving principally to separate or otherwise characterize groups of substances; and (2)

Special reagents, or those which serve principally for the detection of bases and acids. The reagents in the dry way consist of fluxes, and of those employed for blow-pipe manipulations.

The author proceeds afterwards to describe the department of various substances with reagents; this subject is divided into—

A. Department or properties of the metallic oxides and their radicles.

B. Department of the acids and their radicles, with reagents.

Thus ends the first part of the work. It may be considered as an introduction, with which the student must make himself acquainted, in order to be able to understand thoroughly Part II., or *Systematic Course of Qualitative Chemical Analysis*.

This part of the work is divided into three sections. The first includes a systematic course of practical instruction in chemical analysis, which is divided into preliminary examination, solution, and actual examination. The second section contains a detailed account of the special methods employed to effect the analysis—of a few important compounds and mixtures which chemists are frequently called upon to examine; those methods becoming much simplified as the number of substances decrease which are involved in the analysis. The third section contains an explanation of the general analytical processes, with numerous additions to the practical operations, and to which the author more particularly calls the attention of the student. At the end of the volume is a chapter devoted to the department of most of the alkaloids with reagents, together with a systematic method of effecting the detection of those substances. Finally, the author adds some supplementary remarks on the separation and detection of arsenic, antimony, and tin, in presence of each other; in speaking of the methods of determining the nature of organic bases in cases of poisoning, he alludes to the processes employed for that purpose by Stass and Hofmann and Graham. In the former, an alcoholic extract of the parts to be examined is first obtained, with the addition of a small quantity of tartaric or oxalic acid. This solution being concentrated on a water-bath, and the residue treated with carbonate of soda and cold alcohol, a solution is obtained containing the alkaloid; finally, by means of ether, it is separated in a sufficiently pure condition to admit of its properties being determined. This method is therefore principally calculated for the detection of conine, nicotine, aniline, picoline, petinine, morphine, codeine, brucine, emetine, solanine, aconitine, atropine, and hyoscyamine, all of which are vegeto-alkalies soluble in ether. Hofmann and Graham have employed animal charcoal to effect the detection of strychnine in beer, charcoal having the property of absorbing this alkaloid, and yielding it to alcohol.

The systematic method described by Fresenius for the detection of alkaloids is based on the circumstance that certain of these substances possess the property of being precipitated by potash or soda, from their solutions, and of re-dissolving in an excess; others, of being precipitated by these alkalies without re-dissolving in an excess, and of being precipitated also with bicarbonate of soda, even from acid solutions; others again (forming a third group), of being precipitated by potash, and not re-dissolving in an excess, nor being precipitated from an acid solution by the bicarbonate of fixed alkalies.

The analysis of mineral and fresh waters is a subject of particular interest to the medical practitioner, and the method described by Fresenius for that purpose possesses undoubtedly the advantages of being complete, simple, and practical. The first stage of the analysis of mineral waters must be carried on at the well or spring, where the water is to be filtered, and where carbonic acid, sulphuretted hydrogen, and iron may be detected at once. The analysis is now to be continued in the laboratory, the clear filtrate is evaporated to dryness, and the residue treated with alcohol, to separate iodine and bromine; it is afterwards tested, to determine the absence or presence of silicic acid, fluorine, baryta, strontia, arsenic, alumina, and phosphoric acid. Another sample of the water is taken for the detection of lithia; and a third quantity, being previously concentrated, is tested for boracic acid; ammonia is detected with lime by the usual means. The methods employed for determining the presence of other substances which may occur in mineral water, as lime, potash, soda, &c., are described in the paragraph concerning the analysis of fresh waters (spring, well, brook, and river water). The constituents of the substance deposited by the water as it issues from the spring are now to be determined; the mass, previously washed, is treated with hydrochloric acid and filtered; the filtrate may contain baryta, strontia, arsenic, antimony, tin, lead, copper, alumina, phosphoric acid, sulphuric acid, fluorine, and silicic acid, which are separated from each other by a systematic method, and then tested. The residue, insoluble in hydrochloric acid, consists usually of silicic acid, clay, and organic matters, but it may also contain sulphate of baryta, sulphate of strontia, and chloride of calcium; if arsenic has previously been detected in the water, it will be advisable to boil a little of the deposit in a concentrated solution of potash and soda, with the view of ascertaining whether it exists there in the state of arsenious or arsenic acid.

To conclude our analysis of this treatise, we may be allowed to allude shortly to the method described by the author for the detection of arsenic. He recommends a process similar to that which is employed for the detection of grape and diabetic sugar. Where arsenious acid is dissolved in an excess of soda or potash, and mixed with a few drops of a dilute solution of sulphate of copper, a clear blue fluid is obtained; upon boiling, a red precipitate of suboxide of copper falls down: the solution contains arseniate of potash. This reaction the author considers as exceedingly delicate; but it ought only to be employed as a confirmatory proof of the presence of arsenious acid; this method is also useful as a means of distinguishing that acid from arsenic acid. Dr. Fresenius describes minutely the method of detecting arsenic acid, by its being converted into arseniuretted hydrogen. He observes that the spots formed on a porcelain plate by the gas evolved from the apparatus when kindled, have a rather blackish-brown colour and a bright metallic lustre; whilst those of antimony are of a deep black colour, but feebly lustrous. The arsenical stains may be distinguished, moreover, from the antimonial stains by pouring over them a solution of chloride of soda (a compound of hypochlorite of soda with chloride of sodium, prepared by mixing a solution of chloride of lime and carbonate of soda in excess, and filtering), which dissolves the arsenical spots immediately, whilst it leaves the

antimonial spots unaffected, or removes them only after a considerable time.

In regard to the means of detecting the presence of arsenic in articles of food, in dead bodies, &c., for medico-legal investigations, Dr. Fresenius gives a minute account of the process he published some years ago, with Dr. V. Babo, where the solution of the poison is effected by means of hydrochloric acid and chlorate of potash; some sulphate of soda being afterwards added, and then a current of hydrosulphuric acid gas passed through the fluid, the arsenic is precipitated as a tersulphide.

We might add considerably to this abstract of the useful work of Dr. Fresenius; but we trust our remarks will suffice to make the reader acquainted with a general outline of the treatise, which we may recommend as one of the best practical works in the English language upon qualitative chemical analysis.

W. Marcet.

REVIEW XII.

Eutherapeia; or, an Examination of the Principles of Medical Science: with Researches in the Nervous System. By ROBERT GARNER, Surgeon to the North Staffordshire Infirmary, late President of the North Staffordshire Medical Society, &c. &c.—London, 1855. 8vo, pp. 282.

NOT distinctly recognising from the title of this book the precise intention of its author, and imagining that our readers may feel a similar, if not an equal, difficulty, it appears to us most desirable to state at the commencement the object which Mr. Garner has in view, and to do this in his own words:

“To elucidate the credibility of the principles of medicine, to show that these principles may not unfairly be placed in comparison with the accredited conclusions of other kindred sciences, and to vindicate our art amidst the pretensions and dogmas of charlatanism—to meditate for a short time on the origin and sad prevalence of disease, to investigate the degree of curative power furnished us in remedial agents, and to compare diseases and their cures and alleviations with corresponding evils and their remedies in the moral world—to elucidate, too, the existence of a governing power, or degree of inherent curative tendency in the human frame, manifested in the regular course or order of phenomena or symptoms in what are in this respect wrongly called disorders, but, above all, in that happy constitutional aid during their treatment, which we experience in most diseases—to consider to what extent these effects may be relied upon—lastly, the indication of a visible intention and even of goodness in the permission by Providence of disease and pain—such are the subjects which the author would wish to discuss in the following pages.” (p. 2.)

Mr. Garner informs us in the preface, that “he has written for the profession in the first place, but also with an eye to the inquiring portion of the community;” and thus we were led to hail the appearance of his book as that of one, if accomplishing its author’s object, calculated to supply a want which many must have felt it was most desirable to supply.

The idea of the work before us is emphatically good, for perhaps there is no subject upon which the public has more frequently to judge, and upon which it is less generally informed, and consequently judges more incorrectly, than that of medical practice. The numberless vagaries of

pseudo-medical science find adherents, whose faith is firm in inverse proportion to its reasonableness, and whose consciousness of heterodoxy supplies them with a morbid pleasure to set in the balance, against those lamentable results which too often follow from their indiscretion. But it is not only in this direction that the public evinces its want of information; it is also in the erroneous and exaggerated notions which are entertained with regard to the nature of diseases, and the power which medicines can exert for their alleviation; and also in the frequent opposition of its own opinions to those of even its favoured medical advisers. At some of these erroneous ideas we cannot wonder; the public cannot get rid of those links of superstition and false system whose traditionary honour has conferred upon them tyrannical authority; but we must feel astonishment at the credence of many novel absurdities, the slightest examination of which, by the most ordinary rules of logic and common sense, could not fail to demonstrate their utter unworthiness of belief.

Those who are not of the medical profession are so constantly compelled to seek its aid, that comparison and selection are necessary processes for the individual to perform. Since we are not at all disposed to believe that at the present time, nor are we inclined to wish that at any future time, opinion and action are to be guided by the dicta of institutional authority, it appears of great importance that the public should be so instructed that it may form correct judgments, so far as it is called upon to judge. It is utterly impossible that the facts and doctrines of medical science, in their detail, should be appreciated, except by those who have devoted years to their laborious study; but it is possible, and the progress of general education renders it probable, that when a few more generations have passed away, the data for judgment upon all matters which it really concerns the public to judge will be familiar to the minds of all educated men and women. The principles to be applied in the appreciation of facts, together with the general phenomena and laws of life, of disease, and death, may and should form a part of polite education; and we feel confident that when this is accomplished, the pseudo-sciences, and that which is false in legitimate sciences, will be duly disregarded, the true and the useful being alone chosen as the guides and auxilia of humanity. But at the present time this is not accomplished, and it often happens that the exigencies of a certain period demand a special work which was not possible at an earlier, and which would be unnecessary at a later. It is important, sometimes, to forestall the slow progress of information in the mass, by presenting the better classes with results to which the former will arrive by a more gradual process; and this is, we imagine, the object of Mr. Garner in directing his "eye to the inquiring portion of the community."

Thus, then, we are led to consider the idea of the author of 'Eutherapeia' as an extremely good one; the general plan which he has adopted is likewise commendable, but the mode in which the idea is carried out, and the plan filled up in detail, must, we think, render the book of little value to the profession, and almost incomprehensible to even "the inquiring portion of the community." It details few facts with which the profession are not already acquainted; there is no new arrangement of previously established doctrines to give them increased value; while they

are so constantly, almost invariably, expressed in technical terms, that the public will, we fear, form little idea of their meaning.

The book is divided into eight chapters, their subject-matter being respectively—Anatomy, the Nervous System, Organic Chemistry, Pathology (two chapters), Medicinal Substances, the Divine Dispensation in Disease, and the Pseudo-Medical Sciences. A book having this general plan might, we conceive, be made of much value to the profession, and especially to its younger portion, some of whom often commence their studies without any definite idea of the inter-relations of their several branches. It might also be the means of conveying much instruction to the public, in its endeavours to know something of the mysterious organism with which each individual feels himself associated, something of the diseases which may make it an almost intolerable burden, and the objects of these diseases, as parts of a "Divine dispensation," in their relation to the individual, and to those means which Providence has supplied for his relief. Thus there might be gained some direction, not only of external life under the pressure of its immediate physical derangements, but direction of mind and of heart in regard to the exercise and tenure of that life in its bearing upon individual prospects and social claims.

A few quotations from the chapters, the titles of which have been already mentioned, will enable our readers to judge for themselves with what success Mr. Garner has carried out his scheme, and they will, we think, show that the general criticism we have given above is not without foundation.

As a specimen of the chapter on Anatomy, or the "survey of different systems of organs, particularly connected with their functions," let us take that with which Mr. Garner commences, "Digestion, or assimilation." After stating that the food is swallowed, and that it may be watched down the œsophagus of a horse, we have the following:

"The movements of the stomach are, no doubt, curious; we see in animals a result of them, in the hair balls sometimes found there, polished and round externally, internally consisting of hairs which the animal has licked and swallowed. Galen and Vesalius pointed out many peculiarities of this organ, the former its muscular coat, as well as the mucous follicles, which last, however, have received their names in this organ, and in the intestinal canal, from later anatomists—Brunner, Lieberkuhn, and Peyer. According to Bernard, the gastric juice is secreted only in the pyloric third of the stomach. After the food is converted into chyme, the pylorus admits it into the small intestines, or progressively into the duodenum, jejunum, and ileon, and thence it passes into the cœcum, colon, and rectum, or large intestines, divisions distinguished by the ancients. The small intestines deliver their contents into the cœcum by the valve named after Bauhin, previously, however, described by Rondeletius. Achillinus and Béranger had also studied this curious portion of the intestinal canal, the latter, seemingly, viewing it as a second stomach. It is extremely large in some vegetable feeders, as rodents and solipeds. The villosities giving origin to the lacteals, the valvular duplicatures, the mucous follicles, and the muscular coat, arranged in the large bowels in longitudinal bands, also the processes and duplicatures of the peritoneal coat, often containing fat, were described by Vesalius, and other early anatomists." (p. 15.)

After stating that the "chyme is mixed with the bile in the duodenum," and also with the pancreatic juice, the result being "the production of the chyle and fœces from the chyme," we are informed that "in the present day Kiernan and Paget in this country have investigated the

more minute structure of the liver," and then, with a few remarks upon the gall-bladder and spleen, the anatomy of the digestive system is completed, and that of the circulating and respiratory organs is passed under similar review.

The above passage is sufficient to indicate the deficiency of Mr. Garner's book. The account of the stomach and intestines is so trivial, that the youngest student of anatomy cannot but be discontented; he is made acquainted with every fact which Mr. Garner has mentioned during his first session of attendance upon lectures, except, perhaps, the individual merits of Rondeletius, Achillinus, and Béranger; and it is of very little use to tell the public of mucous follicles, the pylorus, gastric juice, and chyme, and that the latter passes through six long names of intestine, unless some definition is given of the terms employed. The remark upon the "more minute structure of the liver" would be, perhaps, more intelligible to the community if something had been told them of its less minute structure, or if a reference had been given to the original papers of Messrs. Kiernan and Paget; but there is nothing of the kind to be found, and the statement is scarcely less complimentary to the tyro in anatomy, than it would be to tell him that Shakspeare had written some dramas, and that Bacon was the author of the 'Novum Organon.' We think it would have been better to have left the liver out altogether, for there certainly is no subject upon which the public holds more erroneous ideas. It is looked upon as a very Moloch of an organ, to be pleased and displeased in a thousand ways, to be set wrong when right, and to be set right when wrong, by means that are totally at variance with anything in the shape of facts, or sound physiological doctrine.

The chapter on the Nervous System is far superior to the others; it contains a succinct and interesting *résumé* of our knowledge with regard to its anatomy in the different classes of animals; and though much more valuable to the student than any other part of the book, is, perhaps, even less so to the public, as it is extremely difficult to avoid the frequent use of technicalities. There are, however, many passages which we think open to misconstruction: volition and perception are not distinguished, as they should be, from motor impulse and sensation; and there is a looseness of phraseology which cannot but give rise to indistinct ideas. As examples, we may quote the following:

"When the organs of motion become more perfect, and sensation less diffused, there must be nerves and ganglia, the latter the seats of volition, or motor impulse and perception; the former the conductors of such impulse from, and of the sensation to, the ganglionic sensorium." (p. 26.)

"From injury of the spinal cord or of a large nerve, it is proved that sensation and motion are destroyed in all parts below the injury, the medium of communication being destroyed." (p. 39.)

"Paralysed limbs are, however, sometimes seen to move when pricked or pinched, without the sensorium being conscious of it." (*ib.*)

It is quite unnecessary to point out the indefinite, if not erroneous, manner in which the words, "motion," "sensation," "perception," and "conscious," are employed in these sentences. It is also, we think, unnecessary to make any comments upon the three pages which contain "pathological conclusions" with regard to "diseases of the nervous system," but we select the following as a specimen: "If the fornix is

affected, the *speech* is imperfect, there is *double vision*, *contractions* and *convulsions of the limbs*, *incoherence*, *loquacity*, *delirium*, *stupor*, and *coma*."* (p. 73.)

The chapter on Organic Chemistry is very defective in many points, but represents with fairness the present position of our knowledge with regard to others. We may mention, in proof of the former observation, the manner in which the question of animal heat is treated; as an example of the latter, the paragraph upon the functions of the liver. We do not think that our author, in speaking of the blood-corpuscles, has been fortunate in his illustration, as the following quotation shows:

"It (the blood) also contains a vast number of flattened globules floating in the serum, which appear to be as much organizations as the cells of the areolar tissue, and, like them, have commonly central nucleoles. On these corpuscles or discs the colour of the fluid depends." (p. 90.)

We do not see that in any previous portion of the book Mr. Garner has described (or has even referred to any one who has described) the "more minute structure" of areolar tissue. If he had done so, we cannot but think that "the inquiring portion of the community" might ask some unpleasant questions; but as he has not, we will not attempt to pry into the analogy between the blood-corpuscles and the "cells of areolar tissue." The chapters on Pathology, are introduced by a history of opinions with regard to the nature of disease; and this, although sketchy, is decidedly the best part of these chapters. There is a want of arrangement, an absence of anything approximating to such a general view of the question that the public might gain any valuable information, whilst the details are so imperfect that the profession can derive little benefit from their perusal.

Inflammation—that great "bugbear," as it has been termed, not only of the community, but of the profession itself—is the first object of which our author treats. It is spoken of as "an exaltation of vital action," and the buffy coat is said to be a "sign which seldom deceives;" but there is no attempt made to present such a view of the process that old and erroneous ideas may be removed. The inflammations of various organs are very briefly commented upon in separate sections, four or five lines being devoted to meningitis, about twice that number to pleuritis and empyema, and so on. We then come to varieties of fever, and under the title of "Continued Idiopathic Fever" find the following:

"Twenty years back, continued fever, in some parts of England, might be termed cerebral, and some considered its essence to consist in inflammatory action of the brain. Laterly it has been much more commonly attended by inflammation of the gastro-enteric† mucous membrane, and not unfrequently with a minute or miliary eruption, or with bronchitis," &c. (p. 128.)

This is all that is said upon the important differences to be observed in the group of continued fevers, and it is only one of numerous specimens of pathology very far behind that of the present day. The following will serve as another example of the same chapter:

"LARYNGITIS.—True inflammation of the larynx is a very serious matter, requiring to be subdued at almost any expense to the system, or else it is necessarily fatal in a short space of time. The great Washington died of this disease, and it has been actively canvassed whether, in the case of an old man like him,

* The italics are Mr. Garner's own.

† A misprint, we presume.

the above rule of practice should not have been much relaxed; he was a hero, and certainly in his last hours appears to have been treated heroically." (p. 185.)

In the sixth chapter, medicinal agents are classified in accordance with their differences of action upon systems of organs; but there is nothing in this classification to demand notice, and we again fail to find, what we should have expected, some general statements of the modes and limits of medicinal operation.

The chapter on the Divine Dispensation in Disease occupies a relation to natural theology similar to that which the preceding chapters occupy to physiology and pathology. The pages of this journal are not the proper place for a discussion of the question raised by Mr. Garner's treatise; for, although a belief in the Divine dispensation of all things, can alone place man in the position from which he may so view, as to appreciate correctly, the phenomena of this world, as displayed in his own life, in that of the daisy at his feet, or in the wider ranges of cosmogony and history, yet the attempt to establish inductively the existence of that dispensation, can be successful only upon the admission of certain premises, the granting of which is tantamount to an admission of the whole; it is still further incorrect in method, inasmuch as it is based upon the consideration of facts, which can be fully appreciated only by the admission of that which it seeks to prove. Allowing the Divine dispensation to be a great and universal truth, the history of man in disease, as well as in health, affords abundant illustration of its presence; but if we possessed nothing beyond the phenomena of earth to teach us theology, if we had no surer convictions of the power, the order, and the guardian care of a Supreme Being, than those which we could obtain by inductive reasoning, the little faith that we now have would become almost extinct, and life would in reality be a dream.

The chapter on the Pseudo-Medical Sciences contains an examination of the principles of hydropathy, homœopathy, &c. The remarks are many of them good, and the treatment of the subject fair, but they are not such as to demand any special notice.

We wish that our task in reviewing this book had been more pleasant; but although it is impossible to avoid seeing that much labour has been bestowed upon it, we cannot regard it as likely to accomplish the very laudable and desirable object which its author had in view, and we can only trust that he may be more successful in some future endeavour to "set the claims" of the medical profession "in a fair position," and to "demonstrate that considerable reliance may be placed upon the present theories and practice of medicine." Mr. Garner has endeavoured to write for the profession and "the inquiring portion of the community" at the same time—and this is always a difficult work to undertake, but perhaps with no subject more so than with the one which forms the topic of *Eutherapeia*.

PART SECOND.

Bibliographical Record.

ART. I.—*The Diagnosis of Diseases of the Brain, Spinal Cord, Nerves, and their Appendages.* By J. RUSSELL REYNOLDS, M.D. Lond., University Medical Scholar, Assistant-Physician to the Hospital for Sick Children.—London, 1855. 8vo, pp. 251.

NOTHING can tend more to remove from therapeutics the reproach of vagueness and want of certainty, than the improvement of diagnosis. Given, even in the present state of medical knowledge, a precise lesion in a tissue, the conclusion as to the powers or inefficiency of a remedial agent is not likely to be far removed from truth. We may not know how to cure the malady, when we have arrived at a precise knowledge of its characters, but we shall at all events know with tolerable certainty how much or how little we are able to do. The diseases of the nervous system at large, more than the affections of other portions of the human frame, require what Dr. Reynolds calls "differentiation," in order to enable the physician to arrive at a satisfactory system of neuro-therapeutics. To the task of classifying and comparing the various symptoms characteristic of diseases of the nervous system, Dr. Reynolds has applied himself with great candour and discrimination, and the result is a book which, while it does not profess originality of matter, brings before the student, in a form that is original and clear, a summary of what may be regarded as the ascertained facts of neuro-pathology. The subject is treated under four heads. In the first Part we find the general pathology of the diseases in question, embracing the objects of diagnosis and its limits, the elements of diagnosis, an exposition of the classification pursued, and the diagnosis of locality generally. The second Part is devoted to the diseases of the brain; the subjects, treated in ten chapters, are as follow: 1. The diagnosis of brain diseases as to their general nature. 2. The differential diagnosis of acute febrile diseases affecting the brain. 3. The differential diagnosis of apoplectic diseases. 4. Differential diagnosis of diseases marked by delirium. 5. Differential diagnosis of convulsive diseases. 6. Differential diagnosis of acute hyperæsthesiæ. 7. Chronic diseases generally. 8. Diseases characterized by exalted activity. 9. Diseases marked by diminution of function. 10. Diseases characterized by the combination of increased and diminished function.

Part the third comprises, in four chapters, the diagnosis of diseases of the spinal cord, under the following heads: The diagnosis of the special

locality affected—The diagnosis of spinal diseases as to their general nature—Acute disease of the spinal cord and its meninges—The chronic diseases of the spinal cord. Two chapters on the diseases of the nerves form the fourth and concluding Part of the work.

As a careful digest of the acknowledged phenomena accompanying the diseases in question, the work of Dr. Reynolds cannot fail to be valuable to the student and interesting to the practitioner.

ART. II.—*Atlas of Cutaneous Diseases.* By J. MOORE NELIGAN, M.D. Edin., M.R.I.A., Honorary Doctor of Medicine Trinity Coll., Dublin, Fellow of the King and Queen's College of Physicians in Ireland, &c. —*Dublin, 1855.* 4to, plates 16.

THE art of chromo-lithography has, in the present instance, achieved a very satisfactory result in the production of coloured illustrations of cutaneous diseases; great credit is due both to Dr. Neligan in supplying to the student so useful a work, and to Messrs. Forster for the manner in which it is executed. Extreme care is necessary in adjusting the various stones required to represent a coloured object, independently of the difficulty of graduating the tints. The latter difficulty is necessarily very great in so delicate a subject as the multiform hues accompanying skin diseases. Another great difficulty in their representation by chromo-lithography consists in the distinction of minute differences—as of small vesicles and papulæ—upon which, however, often the diagnosis depends. In the work before us some of the drawings are particularly close representations of nature; we would instance the first plate, giving the various forms of erythema, and an admirable one of erysipelas of the face; the forms of herpes (plate 4); of impetigo (plate 7). Plate 13 gives a most artistic representation of purpura, which cannot fail to impress the student, so that he would at once recognise the disease at the bedside. Nor are the illustrations of lupus and porrigo favosa less characteristic. We need scarcely add that we cordially recommend the work.

ART. III.—*Unsoundness of Mind in Relation to Criminal Acts. An Essay to which the first Sugden Prize was this year awarded by the King and Queen's College of Physicians in Ireland.* By JOHN CHARLES BUCKNILL, M.D. Lond., Licentiate of the Royal College of Physicians, Fellow of University College, Fellow of the Royal Medical and Chirurgical Society, and Physician to the Devon Lunatic Asylum. —*London, 1854.* Small 8vo, pp. 148.

THERE is scarcely a more melancholy spectacle than that of a member of society being treated as responsible for acts which disease had rendered him incapable of seeing in their true bearing; there is scarcely one that is more calculated to bring the administration of the law into disrepute than that of moral delinquency finding sympathy and palliation under an erroneous plea of mental aberration. As long as the accidental bias of a jury composed of men who have never seriously reflected upon or in any way studied the subject of mental derangements, determines the

momentous questions involved in a plea of insanity, and as long as medical men are put into the witness-box for the purpose of making out a case rather than of eliciting truth, so long we shall be liable to witness the melancholy spectacle of moral guilt unpunished, of innocence subjected to the extreme penalty of the law. The study of insanity is one demanding so much attention and careful study; the operations of the mind are so intricate, and all language defining and delineating them so liable to misinterpretation, that nothing is easier for a skilful advocate than to elicit from a medical witness statements which may suit his own purposes. It is manifestly unjust to place a medical man who has not specially studied the subject of insanity, and does not even know the terminology of psychopathy, in so false a position. In regard to this point, as in regard to many other questions connected with forensic medicine, nothing is more imperatively demanded in this country, than the institution of offices to be held by men qualified by research and practical experience to give, not a judgment, but an authoritative opinion on the matters alluded to. A *master in medicine* would be an appropriate title, and one which, if remunerated as such a post must be to secure the exclusive services of a superior class of men, would be more suited to the genius of the country than others taken from foreign countries. He would correspond to the government physicians or *physici* of Prussia, or the *experts* of France. Upon these and allied points Dr. Bucknill dwells with much force.

The relation of cerebro-mental diseases to the law, and their interpretation in the medico-legal sense, is given by the author in a form which carries conviction with it. The book is one that deserves to be studied by all who may have to deal with forensic questions bearing upon insanity.

We have followed Dr. Bucknill with much interest in his arguments on the fallacies resulting from the practice of determining by solitary interviews with an individual on the intricate questions of sanity and insanity. We had marked several passages for extract, and regret that we are compelled to confine ourselves to referring the reader to the book itself, with our hearty commendation both of its matter and style.

ART. IV.—*Wiesbaden als Heilquelle, und als Climatischer Heilort dargestellt.* Von Dr. C. BRAUN, Prakt. Arzt. Zweite Auflage.—*Wiesbaden*, 1855. 8vo, pp. 144.

The Mineral Waters and Climate of Wiesbaden. By Dr. C. BRAUN.

AMONG the numerous watering-places that abound in the volcanic regions of the Rhine, Wiesbaden has held a prominent place from the days of Pliny downwards. The springs that are employed medicinally are, with one exception, thermal, and their temperature varies considerably—that of the chief brunnen, the Kochbrunnen, which is the highest, is, according to Fresenius, 55° R. (156° F.); the others range from this to 10° R. (54° F.). The main constituents are chlorides of sodium, calcium, potassium, and magnesium, with minute traces of iodide of magnesium, carbonate of iron, and of other salts. More or less free carbonic acid and nitrogen are found in most of the springs.

We draw attention to Dr. Braun's work, because it is distinguished from the great bulk of writings of the kind by the attempt to withdraw balneology from the sphere of crude empiricism. He has performed a series of experiments to determine the physiological action of the waters of the Kochbrunnen upon the system. As we shall shortly have occasion to enter more fully into the question of the metamorphosis of tissues produced by these agents, we now confine ourselves to the following summary of the physiological effects produced by the waters of Wiesbaden.

In the internal administration we are first met by the improved digestive powers. This is produced, in the first instance, by the water, which is demulcent and solvent; in the second, by the chloride of sodium, which is a gentle stimulant, liquefying the mucus and protein compounds; next, by the volatile stimulus of the free carbonic acid, and also by the tonic influence of the small quantity of iron present. In small doses, frequently repeated, the action is confined to the upper portion of the intestinal canal, and to the general stimulation of the urinary organs and lymphatic system. In larger doses the water becomes purgative, and the secretions generally are more powerfully excited. Dr. Braun does not assert any specific action of the waters in definite diseases, but claims for them a more or less powerfully alterative effect in various morbid conditions of an asthenic character. As, in 1854, the number of patients (*kurgäste*) is said to have amounted to twenty-six thousand, many of whom were undoubtedly our own countrymen, it is not a matter of scientific importance alone that we should turn our attention to the physiological and therapeutic effects produced by such waters as those of Wiesbaden.

We may add, with regard to the climate of the place, that Wiesbaden, owing to its protected site at the southern declivity of the Taunus mountains, has the advantage of a more uniform and a higher average temperature than the country generally, enjoys. The atmosphere is of a dry character, and the amphitheatrical form of the adjoining mountains guards the locality much against winds,—a point of considerable importance, when it is selected as a residence for patients affected with pulmonary disorders.

ART. V.—*The Medical Profession in its Relations to Society and the State. An Oration delivered on the Eighty-second Anniversary of the Medical Society of London.* By J. F. CLARKE, Esq., late Vice-President of the Society, &c.—London, 1855. pp. 24. (Published at the request of the Society.)

FROM whatever point of view the present position of the medical profession be regarded, one thing is certain, that we must adopt and act upon the motto, *Pax domi, foris Bellum*. The moral force which we are gradually acquiring we owe to the education, the liberality, the intelligence, of the members of the profession; and by perseverance in the prosecution of objects, which it is good for the commonwealth, more even than for ourselves, that we should obtain, they will be realized sooner or later, because they are the inherent necessities of the development of an important and integral part of the State. How soon that realization shall be effected must depend mainly upon the energy, the temper, the

unanimity, with which the goal is followed. Neither the flippant impertinences of a Prime Minister, nor the supercilious condescension of men who are unable to appreciate the noble deeds of the past, and the achievements and aims of the present generation of medical men in behalf of their country, need for an instant divert the profession from the pursuit of that goal; but above all things it is necessary to be firm and united. Mr. Clarke eloquently dwells upon the points at issue between the profession and the general public,—the improper constitution of all the public boards in which medical questions are paramount, the deplorable system of gratuitous advice, the encouragement of quackery, the absence of all recognition of the profession as a body in the houses of legislature; the absence of public rewards to members of the medical profession, whose services to their country, had they been yielded by others, would have secured to them the highest honours. The sentiments expressed by Mr. Clarke are essentially those of the profession at large, and by promulgating them in the position which was afforded him by being selected the orator of the Medical Society of London, he has added one more stone to the edifice which it is the bounden duty of each individual member of the profession to assist in erecting and completing.

ART. VI.—*Observations on Diseases of the Rectum*. By T. B. CURLING, F.R.S., Surgeon to, and Lecturer at, the London Hospital. Second Edition, revised and enlarged.—London, 1855. 8vo, pp. 129.

WE have on a former occasion expressed ourselves favourably on Mr. Curling's work, and have a pleasure in reiterating that opinion. The present edition contains an additional chapter on the very troublesome complaint, prurigo podicis; Mr. Curling adverts to the various local and general causes to which it may be due, and we may mention that among the local applications, he recommends as one of the best lotions for relieving irritation of the part, one composed of a drachm of sulphuret of potassium and eight ounces of lime water.

ART. VII.—*An Essay on the Action of Medicines in the System, or on the Mode in which Therapeutic Agents introduced into the Stomach, produce their peculiar Effects on the Animal Economy*. By FREDERICK WILLIAM HEADLAND, M.B., B.A., F.L.S., M.R.C.S., &c. Second Edition, revised and enlarged.—London, 1855.

THE rapidity with which the first edition of this work has been disposed of, shows that the subject upon which it treats, is one upon which the profession anxiously seek information; it is *a priori* evidence in favour of the author's merits. The second edition contains additional matter to the extent of about fifty pages. The care with which the literature bearing upon the subject has been studied by the author up to the most recent period, and the use he makes of it, considerably enhances the value of the work. We may instance the questions relating to the theory of endosmosis, in its bearing upon the action of medicines; Dr. Headland, both by his own experiments, and by the evidence he adduces from other

inquirers, further strengthens the strong case he had made out in his first edition, that the purging or diuretic effect of saline solutions depends rather on the quantity of the salt contained in them, than upon their degree of dilution.

Though a work of this kind necessarily contains much that is speculative, and much that will therefore excite controversy, still the earnest and thoughtful labour and candid criticism that the author has bestowed upon the subject, insure to his essay a high rank in medical literature.

ART. VIII.—*A Universal Formulary, containing the Methods of Preparing and Administering Official and other Medicines, the whole adapted to Physicians and Pharmacutists.* By R. EGLESFIELD GRIFFITH, M.D. Carefully revised and much extended by ROBERT F. THOMAS, M.D.—*Philadelphia, 1854.* 8vo, pp. 651.

THIS work presents, in a very complete and well-arranged form, “a compendious collection of pharmaceutic processes and formulæ, with such additional information as may render it useful to the physician and apothecary.” The first seventy five pages contain an analysis of the weights and measures used throughout the civilized world, chapters, on specific gravity, on the temperatures required for pharmaceutical operations, on the specific gravities of some of the preparations of the Pharmacopœias, and on the relation between the different thermometrical scales; an explanation of the abbreviations used in formulæ, a vocabulary of words employed in prescriptions, and some observations on the management of the sick-room follow; the introductory part concludes with some general rules for the administration of medicines, the utility of which we should much question, as they convey too little information to the educated medical man, and ‘would only be likely to mislead the uneducated or the layman. The body of the work contains the simples and chemicals, arranged alphabetically according to their Latin names, and under each the pharmaceutical and other preparations in which they are employed, with such further information as to mode of preparation, dose, and administration, as may be legitimately looked for in a book of this kind. Under the head of addenda are to be found “a large number of practical receipts which could not have been introduced with propriety under any officinal heads,” such as anatomical injections, cements, poisons for vermin, inks, and the like. Recipes for the dietary of the sick-room, a list of incompatibles, a full posological table, and a comparative table of pharmaceutical names employed in the Pharmacopœias of the United States, London, Edinburgh, and Dublin, follow. We then find a long chapter of directions with regard to pharmaceutical processes, followed by one giving the symptoms, post-mortem appearances, and antidotes for poisons. A full and complete general index concludes the work; this is preceded by an index, which is less complete, and therefore very likely to mislead, of diseases and their remedies.

On the whole, the work is an excellent compilation, and the English student will find it useful as a means of becoming acquainted with many medicinal substances yielded by the rich flora of America, besides those with which he is already familiar. The following are a few of those given in Dr. Griffith’s work:

Apocynum Androsæmifolium, a native of the United States, emetic and diuretic; forty grains are emetic; *Cahinea*, the root of *Chiococca Anguifuga*, a Brazilian shrub, a diuretic used in dropsies, in doses of from thirty to forty grains; an alcoholic extract and decoction are given; *Ceanothus Americanus*, a native of the United States, with a bitter and astringent principle, residing chiefly in the root; *Coptis trifolia*, the orange-yellow roots contain a pure bitter, without tannin, and are employed in powder, infusion, and tincture, as a stomachic; the whole herb of *Cunila maritima* is employed in various forms, as a stimulant, carminative, sudorific, and emmenagogue; *Heuchera Americana*, or alum root, a powerful astringent of the order of saxifrages; *Monarda punctata*, a perennial North American herb, abounding in a volatile pungent oil, and employed in flatulent colic and as an emmenagogue; *Phytolaca Decandria*, a large herbaceous plant, the berries and root of which are officinal; the root is emeto-cathartic, with some narcotic properties, and is employed as an alterative in syphilis, rheumatism, and chronic eruptions; it is alterative in doses of from one to five grains, emetic in from ten to thirty; *Podophyllum peltatum*, an herbaceous plant, the dried root of which exhibits the same properties and apparently the same strength as jalap; the resinous principles may be separated in the form of a dry, amorphous mass, and is termed podophyllin.

ART. IX.—*The Essentials of Materia Medica, Therapeutics, and the Pharmacopœias. For the use of Students and Practitioners.* By ALFRED BARING GARROD, M.D., Professor of Materia Medica and Clinical Medicine in University College, London, and Physician to University College Hospital.—London, 1855. Small 8vo, pp. 282.

THIS excellent little book is essentially a class-book, and as such will be found very useful to those who, especially among students, are likely to be bewildered by the larger compendia that treat of materia medica and therapeutics. It would, however, require some additions, in order to adapt it for the Scotch or Irish student, as little account is taken of the Pharmacopœias of the sister countries. We probably express a very general wish felt by the profession in saying, that we hope a 'Pharmacopœia Britannica' may be in existence before a second edition of Dr. Garrod's book is called for, and thus render unnecessary the addition of all the variations contained in the existing Pharmacopœias.

ART. X.—*Introductory Lecture, delivered to the Class of Military Surgery in the University of Edinburgh, May 1st, 1855.* By Sir GEORGE BALLINGALL, Regius Professor of Military Surgery.—Edinburgh, 1855. pp. 44.

IT is gratifying to receive the testimony of so distinguished a representative of the military department of medical science as Sir George Ballingall, to the ability and foresight of Dr. Andrew Smith in reference to the present war, and to find it to be his opinion, with which we coincide in the main, that the only defect of the medical department con-

sisted in an excess of good nature in mixing itself, or permitting itself to be mixed up, with the duties, and saddled with the responsibilities, of another department. Sir George specially alludes to the purveyor's department. It is manifestly unjust to expect one man efficiently to attend to the sick soldier, and at the same time to provide the supplies, either of food or physic.

Sir George implies that the excess of good nature prevented the medical officers from acting with that independence, and raising their voices against abuses, with that energy which they might have employed, not only with justice to themselves, but with a certainty of the legitimate object being gained.

"In former days," he says, "I have known a successful representation to issue from the weakest voice in the department. I have known an assistant-surgeon of three years' standing to bring down the censure of the Government upon the medical storekeeper at one of the Presidencies of India, for hesitating to supply him at once with the articles he required. The young man's requisition was sent back to him for amendment, but instead of doing this, he stated the facts to his commanding officer, saying that the requisition should stand upon record, and if the storekeeper was unable or unwilling to comply with it, it was for him to say so, and to state the reason why. The colonel, who at the time commanded a large army about to take the field, galloped off with the correspondence to the Governor, by whom the storekeeper was reprimanded, and the medicines were in camp in less than twenty-four hours."

Had such moral independence always characterized all the members of the profession, in or out of the army, we should not now hear of the "degradation" of the profession (a term to which we, however, do not subscribe), nor would the occasion have been given for the introduction of the civil element into the army, which Sir George loudly deprecates.

We cannot enter further into Sir George's treatise than to express our admiration of that generous feeling with which he speaks of eminent men of an earlier as well as a more recent date, in the military medical service of our country.

ART. XI.—*Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of 1853.* (Published by authority of the City Council of New Orleans.)—*New Orleans*, 1854. 8vo, pp. 542.

IN consequence of a severe outbreak of yellow fever in New Orleans in 1853, the municipality took alarm, and established a sanitary commission, of which Dr. Barton, the main author of the above Report, was the chairman. The commission, as soon as the pressure was removed, was, as we have been given to understand, allowed to expire, but fortunately not before the Report, which is a monument of zeal and industry, was published. We now only give the title and advert to it, in order to introduce it to the notice of all sanitary reformers, lay and medical; as the facts and deductions accumulated in the volume, though immediately bearing upon yellow fever, have a much wider range. Neither time nor space will allow us to attempt even a summary of its contents at present, but we hope ere long to lay before our readers an analysis of the labours of Dr. Barton and his coadjutors.

PART THIRD.

Original Communications.

ART. I.

On the Varieties and Metamorphoses of Chronic Pulmonary Tubercle. By C. RADCLYFFE HALL, M.D., F.R.C.P.E., Physician to the Hospital for Consumption, and to the Institution for Ladies with Diseases of the Chest, Torquay.

(Continued from No. 30, p. 501.)

EXCLUDING the results of softening, pulmonary tubercle is found in different instances under the following forms:—Bayle's semi-transparent granulation; miliary grey semi-transparent tubercle; grey tubercle in various shades of whitish or buff opacity; circumscribed yellow tubercle; circumscribed gelatinous tubercle; diffused infiltration of tubercle, grey, or yellow, or gelatinous.

Bayle's Granulations.—Considerable discussion respecting the exact nature of the granulations described by Bayle might probably have been spared by mere reference to his original account of them. Bayle designates his second species of phthisis "Granular Phthisis." He states that in this form of the disease—

"The lungs are stuffed with transparent, shining, miliary granulations, which appear to be of a cartilaginous nature and consistence. Their size varies from that of a millet seed to that of a grain of wheat; they are never opaque, and they do not dissolve. These different characters perfectly distinguish them from miliary tubercles, which are of the same size, *but which are always grey or white and opaque, and in the end totally dissolve.*"*

This granular form of phthisis constituted more than a fifth of Bayle's cases, being found in 183 out of 900 autopsies of phthisis. It is evident that under this designation Bayle includes every instance in which he found the bright, transparent, hard nodules in the lungs, whether these were associated or not with other forms and conditions of tubercle. In each of his fully narrated cases in which the granulations existed alone, death resulted either from hæmoptysis or from pulmonary congestion, or else from some cause foreign to the tubercles. In every case in which the death was due to phthisis pursuing its more ordinary course, the granulations were in company with common tubercle. It is clear that an object which Bayle met with so frequently as once in every five inspections must be something with which we are sufficiently familiar.

* Recherches sur la Phthisie Pulm. Paris, 1810.

Bayle's statement contains an observation and two opinions. The one opinion is that the nodules appeared to be of a cartilaginous nature. By this we need scarcely now understand more than that in density these granulations were almost as hard as cartilage; just as elsewhere Bayle several times observes, "they are very hard, like small hailstones." At that time it was customary to view every morbid product which was at all like cartilage in density or other outward character, as an accidental cartilage; and, indeed, by the naked eye it is difficult to distinguish the condensed fibrous tissue around a concretion or an old abscess from some forms of cartilage. The other opinion is, that the transparent nodule never softens; although it is also stated, that "these miliary granulations occasion at length ulcerations of the parenchyma of the lungs." More recently, Rokitansky likewise asserts emphatically that his "simple fibrinous tubercle—grey tubercle, the grey tuberculous granulation of Laënnec"—never softens.*

Putting aside these opinions, what is the nature of the objects really observed by Bayle? The answer, as given by Broussais, is, minute pulmonary lymphatic glands, enlarged! Andral considered them to consist of old inflammatory exudation, which had originally been poured out into distinct air-vesicles (vesicular pneumonia); and he imagined that they had passed through a previous stage of softness and redness.† He was evidently misled by the appearances seen by him in the lungs of glandered horses, which he everywhere confounds with true tuberculous disease. The vesicular pneumonia of infants does, it is true, at first glance, appear like disseminated tubercles, but the resemblance is to crude yellow softening tubercles, and not to transparent hard granulations. Rokitansky remarks, that when croupous pneumonia leads to vesicular tuberculous infiltration, "it is the same thing as Bayle's pulmonary granulations, regarding which there has been much discussion."‡ And in an excellent recent work, 'On Pathological Anatomy,' the granulations of simple hepatised lung in non-tuberculous pneumonia are styled "Bayle's granulations."§

The confusion has arisen mainly from Bayle having carefully distinguished his "miliary granulations" from his "miliary tubercles" of the same size. He appears to have called "tubercles" such nodules only as were somewhat opaque. "Tubercles," he says, "are always opaque, of a white or dirty-white colour, at one time yellowish, at another greyish." A seed-like transparent nodule, which we should now designate a grey miliary tubercle, before its stage of opacity, Bayle would not probably have called a tubercle at all, but a "semi-transparent granulation." What we should now style a more or less opaque grey tubercle, is Bayle's tubercle *par excellence*. Laënnec considered Bayle's granulations to be tubercles in their earliest stage; and pointed out that in Bayle's fourth case many of these granulations, which "were more numerous in the superior lobes than in the inferior, and resembled small grains of hail," were opaque in the centre.

By Bayle's granulations, then, is to be understood nothing more than

* Sydenham Society's edition, vol. i. p. 296.

† Clinique Médicale, tom. ii. pp. 10, 11.

‡ Vol. iv. p. 103.

§ Jones and Sleveking's Pathological Anatomy, p. 422.

isolated, often thickly-disseminated, grey tubercles, which happen to be unusually small, hard, and transparent.

Miliary Grey Tubercle.—Seed-like semi-transparent grey tubercles may be found in small number in the apex of one or both lungs, the remainder being free from tubercle; or, strewn as innumerable isolated grains throughout a great portion of one or both lungs; or, clustered in several close groups. Whether disseminated or grouped, the tubercles may appear to be immediately surrounded by healthy lung. It is crepitant, and neither inflamed nor discoloured. On the other hand, grey tubercles may occasionally be found set in lung solidified by any form of inflammatory exudation. Semi-transparent grey tubercles in apparently otherwise healthy lung, without any other kind or stage of tubercle co-existing, are only met with in cases where the patient has been cut off by something foreign to the tubercle, as, for example, in the case of death by accident. Still, in most autopsies of phthisis, specimens may be found of grey tubercles in portions of lung comparatively normal.

Each miliary grey tubercle comprises several air vesicles filled with transparent tubercle, the divisional walls of the vesicles yet remaining cemented in the tubercle. The tuberculous material itself presents cells in great abundance, and free molecular matter in small quantity. Compound tubercle cells and tu-



Fig. 13. Semi-transparent grey tubercle.

- a. Pulmonic fibres seen distinctly amidst the glassy tubercle cells, and surrounding them.
 b. Fatty epithelium and compound tubercle cells, loose near margin of tubercle.—(Mills.)*

bercle corpuscles abound, and are set in a homogeneous matrix; the whole, both cells and matrix, having the transparency of isinglass. In the smaller, harder, and more transparent forms (Bayle's granulations), the matrix possesses a faint fibrillation, and contains proportionably fewer cells (see Fig. 10).† It never forms real fibres, and its fibrillation of surface is quite distinguishable from the bold pulmonic fibres. In the adjoining air-vesicles the epithelium has undergone the degenerative changes already described, but the oil-dots are smaller and more equal, and less altered by ether, than in the case of yellow tubercle.

When a semi-transparent tubercle becomes opaque, it assumes a dull white colour, but does not at first lose anything of its firm resistancy. Nor as it proceeds towards softening does it become cheesy, like the tubercle which is yellow from the first. Examined when opaque but still solid, the whole tubercle has lost its transparency, the cells of every kind have largely disappeared, oil-like molecules are very abundant, and

* The names attached to several of the woodcuts refer to the cases by which the specimens drawn were furnished. They are sufficiently altered from the real names to escape recognition.

† British and Foreign Medico-Chirurgical Review, No. 30, p. 493.

the pulmonic fibres no longer perfectly mark out the natural arrangement of the air-vesicles, but are spread out and partially broken up amongst the rest of the tubercle.

Primarily Yellow Tubercle.—Tubercle, yellow from the first, is found in small and seed-like deposits (miliary yellow tubercle); or, in distinct circumscribed masses, commonly larger than the largest circumscribed grey tubercle; or, in irregular masses larger still. It is dull, opaque, of a whitish-yellow, or a canary-yellow colour; tough and leathery in consistency; or unctuous and cheesy; or dry and friable. Less hard than grey tubercle at any period short of actual softening, it is more readily examined with the microscope.

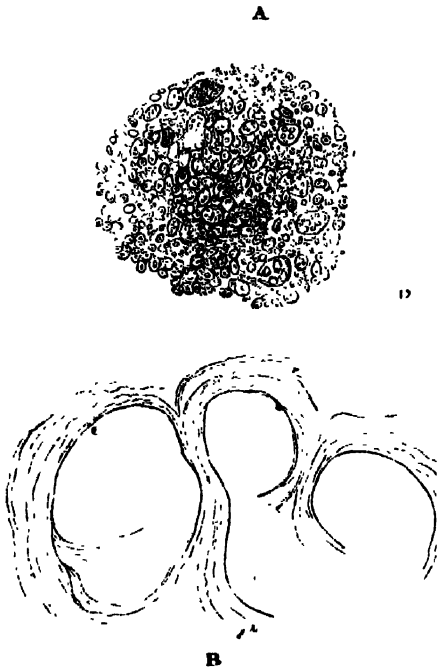


Fig. 14. Crude yellow tubercle.

- A. Tough, brittle, structureless matrix. Free granules more abundant, large cells less so, than in grey tubercle. Tubercle-corpuscles non-nucleated, and more thickly studded with granules. Whole more opaque. ($\frac{1}{4}$ -inch power.)
- B. Skeleton fibres of pulmonic air-vesicles in midst of crude yellow tubercle, tubercle-corpuscles having been floated off. ($\frac{1}{4}$ -inch power.)—(Ford.)

soft, the pulmonic fibres have disappeared, and the molecular matter has increased.

What is the relationship between the yellow and the grey tubercle? Two opinions are current. The one more generally adopted is that of Laënnec, that "there is no other difference between them than that which exists between the green fruit and the ripe." The other is that most prominently supported by Rokitansky, who declares positively and impressively that grey tubercle never softens; that when it appears to do so, it is really because yellow tubercle has been mixed up with it, and that this yellow portion alone undergoes softening. Consequently, that the grey tubercle never changes into the yellow. Bayle also, as we have seen, believed that his grey semi-transparent granulations never softened. It

Yellow tubercle consists of numerous tubercle cells, both compound and single, but the latter greatly predominating; and of a large proportion of free granules, in a homogeneous matrix; the whole being muddy or nebulous. The pulmonic fibres are less noticeable than in grey tubercle, owing partly to the want of transparency, but chiefly to the greater readiness with which a minute portion of yellow tubercle can be removed from the texture of the lung for the purpose of examination. Whilst the yellow tubercle is still consistent, tough, and leathery, the pulmonic fibres do exist within it; but under examination they pass to one side of the object-glass, and the tubercle elements to the other, and thus occasion the latter to appear less connected with the former than had really been the case. When the tubercle has become cheesy, either dry and friable or moist and

soft, the pulmonic fibres have disappeared, and the molecular matter has increased.

may be at once conceded that grey semi-transparent tubercle never does liquefy *as such*. It invariably loses its transparency, and assumes more or less of a dull whitish colour before it softens. Upon the following points every one is agreed. Tubercle may be yellow from the first, or grey and semi-transparent at the first. Sometimes (though very rarely) no other than grey semi-transparent tubercles exist. Commonly, in the same lung with the semi-transparent tubercles we find others which are partially transparent, partially opaque; others which are opaque throughout; some which are softened in the centre, the softening being invariably surrounded by opaque, never by transparent, tubercle; and others again entirely liquefied. Now the moot point is this: is the opaque part of a grey tubercle about to soften really yellow tubercle laid down as such at the first, or so laid down in the substance of the grey after its first formation; or is it merely the grey tubercle itself undergoing a certain kind of molecular change? And, if we assume the latter, does grey tubercle when so changed become precisely the same as primary yellow tubercle? The reply to the last question, if in the negative, will at once decide the first two. Grey tubercle may become opaque, and soften, and yet be a different product all the time to primary yellow tubercle. I believe that this is the fact, and chiefly because the primary yellow tubercle and the opaque portion of grey tubercle are not quite identical. Yellow tubercle always becomes moist and cheesy before it liquefies. Opaque-grey tubercle does not, but remains tough and coherent up to the moment of softening. Softening yellow tubercle is more unctuous. The pulmonic fibres remain intact and entangled amongst the tubercle elements later in the opaque-grey than in the yellow tubercle. The yellow tubercle is, from first to last, more fatty than the other. It is in the air-vesicles adjoining primary yellow tubercle that the fatty degeneration of the epithelium is most characteristic. The oil-dots are larger, more unequal in size, and are more cleared up by ether than in the case of grey tubercle. When an opaque-grey tubercle is about to soften, but is still tenacious and tough, it presents a multitude of even-sized small oil-like molecules, which brighten on the application of ether, but are without ether less nebulous than the molecules which abound in softening yellow tubercle.

Thus, the difference between a tubercle yellow from the first, and an opaque tubercle which was in the first instance transparent, is, that the yellow tubercle is yellower than the other, less resistant, and in composition more fatty and molecular.

Rokitansky's opinion, so often and so positively reiterated, that grey tubercle never becomes opaque and never softens, is open to question by inference from his own remarks. He states, "The combination of grey with yellow tubercle is frequent. Where, in this combination, the *latter* passes into softening, the grey tubercle, like textures in contact with tubercle-pus, becomes destroyed."* He admits, therefore, that grey tubercle may undergo a kind of destruction, which can only take place by liquefaction and molecular disintegration; and this, when effected slowly, is precisely the process of ordinary tuberculous softening. He also states that a grey tubercle may become "a partly cornified, partly ossified nodule." Now, when a tubercle becomes calcified (for, of course, the term ossified is only a verbal inaccuracy), its elements have previously

undergone a molecular change of the same nature as that which in another instance, where it takes place more rapidly and is attended with more liquefaction, constitutes common softening. In these two facts, therefore, as stated by Rokitansky—viz., that grey tubercle can “become destroyed,” and can undergo cretification—there is evidence in favour of its capacity for softening, and if so, of its capability of undergoing the antecedent alterations of opacity and change of colour. On the other hand, there is nothing but the assertion adduced in support of the opposite opinion, that all softening of grey tubercle is due to its concurrence with primary yellow tubercle. One cannot help suspecting that even such an observer as Rokitansky, in supposing that the fibrinous tubercle, as he terms it, never softens, may unconsciously have been biassed by his theory of the necessary connexion of tubercle with fibrinosis. “In the first place,” says he, “the groundwork of rapidly solidifying tubercle blastema is, without the least doubt, fibrin. Again, in the two cardinal forms of tubercle, it is easy to recognise the two principal forms of fibrin, the simple and the croupous.”* Accordingly, he styles grey tubercle the fibrinous; yellow tubercle, the croupo-fibrinous variety. And no doubt there is some broad analogy between the grey and yellow forms of tubercle respectively, and the two leading varieties of exudation-lymph (the nucleated-blastematous, and the corpuscular), so fully described by Mr. Paget.† In grey tubercle, there is more of blastema; in the yellow, more of low corpuscular forms. As the blastematous lymph is better than the corpuscular, so in all probability is grey tubercle a less degraded form than the yellow. It has long been a prevalent opinion, that yellow tubercle betokens a more intense cachexy than the grey. Against it may be urged that we sometimes find tuberculization more universal throughout the body when grey tubercle predominates, than when yellow. But this is readily explained away by the greater chronicity of grey tubercle having allowed time enough; whereas copious yellow tuberculization of the lungs usually kills quickly. When its course is sufficiently chronic, there is no want of extension to other organs on the part of yellow tubercle.

The twofold condition of the part affected and of the constitution, never to be disunited in our reasonings upon phthisis, must be considered to be as influential in governing the exact kind of tubercle deposited, as it is known to be in the instance of common exudation-lymph. It is therefore only what we should expect, seeing how greatly in the course of chronic phthisis both the nutrition of the lungs and the state of the constitution are modified, that the majority of cases would present several forms of tubercle in correspondence with the several stages of the disease. This is so. In an average instance of tuberculized lung, we see enough to justify our assuming that tuberculization has taken place in the following order:—First series in a point of time: Miliary grey semi-transparent tubercles; firm opaque-grey tubercles; the same beginning to soften. So far no inflammation. Opaque-grey tubercles in more advanced stage of softening, now surrounded by inflamed lung. Grey glistening indura-

* Syd. Soc. ed., vol. 1. p. 298.

† Lectures on Surgical Pathology, vol. 1.—The affinity between lymph and tubercle is drawn much more close by Dr. C. B. Williams. See Principles of Medicine, art., Tubercle.

tion, with or without miliary grey tubercles here and there strewn through it; perhaps, a certain number of small tubercles, hard, and partially; or completely, calcified: others softer, white, and mortary. Cavities.—Second series:—Miliary or larger-sized distinct yellow tubercles, surrounded by inflammation; or, masses of the same. Cavities.—Third series:—Jelly-like circumscribed tubercles, or diffused gelatinous infiltration.

The two varieties are variously combined, but in most autopsies of phthisis one is able to say that either the grey or the yellow tubercle predominates.

Can we anticipate this information during the life of the patient? The reply must be furnished by a comparison of symptoms with *post-mortem* appearances. I suppose the following remarks to express the probable truth; they at least serve to represent facts which have occurred in the coincidence stated.

In one case (*grey tubercle*) we shall find a few large cavities, or one immense cavity and several lesser ones; multitudes of miliary grey tubercles; large patches of grey induration; gelatinous infiltration; local spots of oedema and emphysema; plenty of opaque tubercles, but none cheesy or quite yellow. Working remnant of lung in state of purple congestion. Most of the lung externally iron-grey in colour, very contracted, and pimples over, where not adherent, with semi-transparent nodules.

In another case (*yellow tubercle*) we shall find the lungs less contracted generally, bulged forward in places by masses of yellow tubercle; elsewhere, dotted over with yellow nodules, having purple or damask lung between. Cavities less ample but more numerous; and evidences everywhere of more acute and diffused pneumonia.

In a third case, in which the stress has fallen upon the abdominal viscera, there may be only the miliary grey tubercles present in uninfamed lungs; whilst there is yellow tubercle in Peyer's patches.

When grey tubercle predominates, there is more frequently transparent tubercle dotting the peritoneum, and merely an unduly nebulous or granulous state of the hepatic and renal cells.

When yellow tubercle predominates, there is more commonly fatty hyperteliation within the bronchial and mesenteric glands; perhaps more connexion with enlargement of the external lymphatic glands; and fatty heart, liver, and kidneys. This last remark is not intended to express any positive rule. We see cases in which there is fatty kidney with no other than grey tubercles in the lungs; and others, in which yellow tubercle exists in the lungs without fattiness of either liver or kidney. I merely wish to imply a qualified affinity. Whether the lardaceous disease of liver and kidney bear any relationship to one rather than to the other type of pulmonary tubercle, is not ascertained. However unprecise it may be to assert that the cells of the liver are rarely found to be quite healthy in appearance in chronic phthisis, if no abnormal exudation or marked alteration of structure exist to substantiate the opinion, I nevertheless believe that in those cases in which the liver, to the naked eye, seems healthy—being neither fatty nor lardaceous, inflamed, greatly congested, nor tuberculized—the hepatic cells will still

habitually be found more loaded with their ordinary contents than in the perfectly healthy liver.

When consumption is very chronic, for the most part non-febrile, attended with considerable retraction of chest, and with very slow, alternating, but eventually progressive emaciation, and is comparatively free from severe pain of a persistent character, it may be predicted that grey tubercles predominate.

When consumption is more acute, febrile continuously, with little retraction of chest, attended with persistent pain and more hurry of breathing, it is probable that yellow tubercles predominate.

These, like all general remarks, require some qualification. We may see a case where there is intense shortness of breath, and yet the grey tubercles predominate. Here we have, generally, a large cavity in the lung, with a greatly congested and very inactive liver. A very small amount of pulmonary tubercularization, conjoined with an inactive liver, will be attended with more distress of breathing than far more extensive tubercularization where the liver does its duty well. The evil is augmented if at the same time the other supplementary respiratory organ—the skin—be dry and harsh. I have lately attended a case in which sub-acute hepatitis, with great dyspnoea, constant cough, and night sweats, occurring in a young lady aged nineteen, had been mistaken for acute phthisis. It was impossible to decide at the time that latent tubercles did *not* exist in the lungs, but the present good health of the patient negatives such an assumption.

Pain of chest, whether neuralgic or pleuritic, is generally more urgent before than after softening. It by no means necessarily indicates which is the worse side as regards the lungs. There may be sharp pain complained of on the left side, where, on examination, merely coarse respiration is heard; whilst over the right apex, where there is no pain, moist crackling is extensive. Slight local pleuritic irritation on the left side may be the cause here. But immense pleural adhesion may occur over large cavities when grey tubercle predominates, without occasioning any pain. Whereas, when yellow tubercle predominates, there may be less extensive adhesion, and yet constant severe pain of chest. The more acute nature of the inflammatory action, and the greater irritability of the nervous system, in the latter variety, will perhaps account for the difference.

To recapitulate:—We have reason to conclude that the grey semi-transparent tubercle can soften; that previously to softening it always becomes opaque; that this opaque stage is due to molecular alteration in the elements of the previously semi-transparent tubercle, and not to a combination from the first with yellow tubercle, nor to any subsequent addition of yellow tubercle; that the grey tubercle can thus proceed to complete softening without becoming precisely the same thing as primary yellow tubercle, although in both the process of softening is essentially the same; the difference being that primary yellow tubercle throughout its entire course, both before and after softening, has more of oil molecules and granular detritus than the grey tubercle.

Circumscribed Gelatinous Tubercles.—In acute phthisis we sometimes find distinct small deposits, of the consistence only of firm jelly, set in

highly-congested lung. They consist of the same elements which we find in the more common gelatinous infiltration—viz., common grey tubercle-elements, in a more plentiful and softer matrix.

Tubercular Infiltration.—“Miliary tubercles,” say Barthez and Rilliet, “by their agglomeration form masses as voluminous as those of infiltration, and it is only by the presence or absence of the surrounding vascular network that we can distinguish between the two.”* Following Van der Kolk and Guillot, these authors describe miliary tubercles as being surrounded by a fine network of newly-formed vessels, which serves to isolate the tuberculous matter (which they consider it is probably destined to secrete) from the tissue of the organ. These vascular networks were imagined by Guillot to anastomose, through the intervention of pleural adhesions, directly with twigs from the intercostal and internal mammary arteries, and thus to surround each tubercle with arterial blood direct from the heart, in place of that normally found in the system of pulmonary capillaries, which, when such an obstruction as that of tuberculous deposit lay in front, would necessarily be venous. Unable to trace any such network around certain irregular masses, Barthez and Rilliet applied the term infiltration, as above, exclusively to these. • But the whole, possibly with a few exceptions, is a fable. The only vascular arrangement to be found surrounding a miliary tubercle consists of the pulmonary capillaries pushed aside somewhat by the deposit, when this is primarily limited to a few connected air-vesicles. A more diffused exudation taking place, at once encloses all the vessels, and hence infiltration is not surrounded by any ring of capillaries. Rokitansky applies the term infiltration to yellow tubercle, which he considers to be formed in the cavities of the air-cells, whilst grey tubercle (according to him) is formed in the interstices of the pulmonary tissue. He uses the term interstitial as synonymous with grey tubercle; infiltrated as synonymous with yellow. Even were the anatomical facts correct (which I believe they are not), if we mean by infiltration the filtering of a morbid fluid into and amongst the elements of a texture, we should reverse this assignment, and call that tubercle infiltrated which was interstitial, rather than that which was only vesicular. In common use, the term infiltration of tubercle is vaguely employed to signify during life either the sudden supervention of acute phthisis, or else any rapid addition to solidification in a lung known or suspected to be tuberculous. And in morbid anatomy, it is customarily applied to any consolidation of lung of which tubercle forms a part.

Under this designation have thus been comprised—1. The sudden pouring out of tubercle into multitudes of air vesicles, with intense congestion and sanguineous œdema of the whole lung (acute tuberculization). In this case, strictly speaking, the term is a misnomer; serum being infiltrated, tubercle restricted to the air vesicles. 2. One or other kind of simple inflammatory exudation, extensively diffused around pre-existing tubercles. 3. A large mass of yellow tubercle monopolizing all the elements of the portion of lung affected. 4. A conjoined diffusion of tubercle corpuscles, and some kind of simple exudation.

In a strict sense, every ordinary pulmonary tubercle is, to a certain

* *Maladies des Enfants*, tom. iii. p. 8.

extent, an infiltration, since, whilst it begins in the air-vesicles, it subsequently invades the interstitial tissue also. But so long as it is distinctly circumscribed, the term is not applied. By tuberculous infiltration, then, we understand every kind of diffused exudation in which tubercles are contained, whether in distinct nodules, or molecularly distributed throughout. Every kind of diffused consolidation in a tubercularized lung (pulmonary apoplexy excepted), is a consequence of inflammation. Tuberculous infiltration, therefore, is simply tubercle mixed up with inflammation-

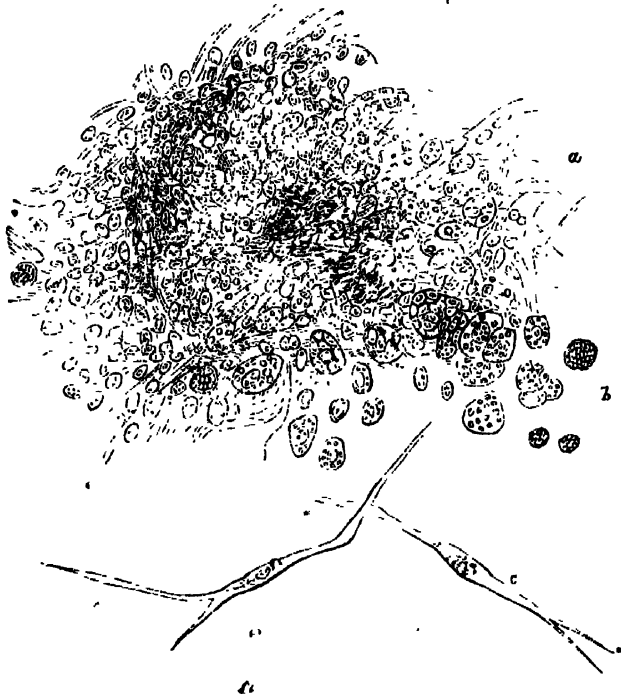


Fig. 15. Infiltration of grey semi-transparent tubercle, made up of fibrillated matrix, imbedding—

- a. Numerous cells, compound and free, nucleated; few non-nucleated; few free oil-dots; few granules.
- b. Glomeruli; fatty epithelium; compound tubercle cells, nucleated and non-nucleated cells; seen loose at margin.
- c. Transparent fibrils, loose, in process of development.—(Weale.)

and are represented only by the interspaces which exist amongst the bands of fibres. Distinct tough grey miliary tubercles may or may not be sparsely or thickly present, and may stand out in relief on cutting through the part; but neither by section nor by tearing can any other kind of granulousness be produced. A thin colourless serum bedews the part. No bloodvessels and no bronchial tubes remain pervious within the centre of the mass. Small bronchial tubes may be pervious for a short distance beyond the margin of the induration. If so, this part of them contains mucus, and has its lining membrane thickened, softened, and sometimes reddened. The lung bordering the grey induration may be in the state of red hepatization, but more frequently it is crepitant, mottled black, blue and red, and only moderately congested.

matter;* and its consideration necessarily involves that of the mutual relationship customary between inflammatory exudations and tubercle.

Diffused tuberculous solidification in the lung is grey and hard; yellow and firm; or soft and gelatinous.

Grey Infiltration is found in large smooth patches, contracted, tough, shining and slippery. To the naked eye, the texture of the lung in it is lost; but under the microscope, the pulmonic fibres are seen cemented irregularly in the semi-transparent exudation. The air-vesicles have lost their normal form and relative arrangement,

* See Cyclopædia of Anatomy and Physiology, vol. iii. p. 105, art., Products, Adventitious, by Dr. Walshe.

In the midst of the semi-transparent induration, we may find distinct grey miliary tubercles; the same in their opaque stage; spots of cheesy-yellow tubercle; and *depôts* of softened tubercle. Or, on the contrary, a portion of tuberculous lung may be glazed with grey, smooth, semi-transparent induration, in which no distinct tubercles are to be seen.

In the lung which is close adjoining grey induration, we find nebulous and fatty epithelium; compound tubercle cells and granule cells. In the induration itself, a translucent homogeneous, or else faintly fibrillated matrix; a few fibre cells, partially developed; the pulmonic fibres matted together; and numerous cells of various sizes, nucleated and unnucleated. Of these cells, many are the round granule cells, only differing from those common to all inflammatory lymph in being more transparent. When distinct tubercles lie in the induration, they add, of course, their own proper microscopic appearances.

Yellow Infiltration is also found in large irregular patches, but is not contracted, and is less tough than the grey; being rather unctuously tenacious, or even friable, than firm, coherent, and resistant. The adjoining lung is always inflamed, and is more commonly the seat of local red hepatization than is the case around grey infiltration. We find a greater abundance of free granules and of oil molecules; of cloudy granule cells and tubercle corpuscles. The matrix is never fibrillated. The pulmonic fibres are less distinct, and apparently in part destroyed. The whole is semi-opaque and muddy.

Gelatinous Infiltration is pink, yellowish, or ash-coloured. The part of lung thus affected is smooth and softly solid, but compressible. The air-vesicles within it retain their normal arrangement, excepting where miliary tu-

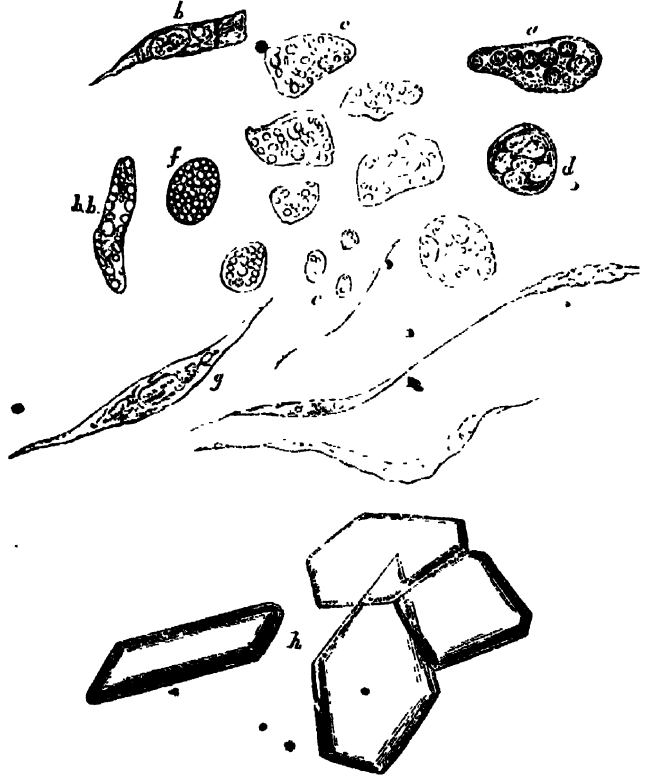


Fig. 16. Gelatinous infiltration.
Constituent elements of grey, firm, gelatinous tubercle.—(Gedd.)

- a. Large pigment cell.
- b. Bronchial epithelial cell, with its cilia extended, and so cemented in the matrix.
- b. b. Bronchial epithelial cell, fatty.
- c. Fatty vesicular epithelial cells.
- d. Compound tubercle cells.
- e. Simple tubercle-cells.
- f. Glomerulus.
- g. Fibre cells, developing.
- h. Bright colourless crystals.

bercles happen to exist; or it may be softly nodulated, from the gelatinous exudation being firmer in some of the air-cells than elsewhere. The colour depends merely upon admixture with extravasated red blood-globules, and their dissolved hæmatin. That regarded, we find indifferently in the reddish and the more colourless forms the same objects: fatty epithelium; compound tubercle cells; tubercle corpuscles; pigment cells; granule cells; fibre cells; and sometimes crystals of mineral salts.

These tuberculous infiltrations follow the usual rule of parenchymatous inflammations, by which an acute inflammation tends to soften, a chronic one to harden.

As to their *rationale*:—In the dense, contracted, glistening, grey induration, we see a very chronic inflammatory exudation of common induration-matter, variously mixed up with the elements of tubercle. We may assume that the plasma, when exuded, is very contractile, and but slightly tuberculous: more blastematous than corpuscular.*

In yellow infiltration we see an acute inflammatory exudation; the plasma not contractile, largely tuberculous; more corpuscular than blastematous.

In gelatinous infiltration we see an acute exudation, but thinly adhesive. An effusion of liquor sanguinis, too feebly contractile for the lymph to separate itself as much as usual from the serum. We may infer that it is poured out rapidly, and that it coagulates to its jelly-like extent immediately, from the fact of the chance-setting within it of a bronchial epitheliur cell, with its cilia extended (*b*, fig. 16). The plasma is poor in fibrin, but more blastematous than corpuscular.

Greyness indicates that the exudation is chronic; yellowness that it is acute; jelly-like consistency that it is recent. We constantly find specimens of all in the same lung. The grey induration is the oldest, and represents the earliest reactive response to the irritation occasioned by the tubercles, whilst there is yet a fit state of blood to pour out contractile plasma. The yellow infiltration points to a deteriorated condition of blood, and to more of local inflammatory process. It occurs subsequently to the grey, when the progress of the disease had injured the constitution more. The jelly is a still later exudation, and takes place towards the close of life, when the blood has become poor in every respect, and unable to furnish firmly-coagulable lymph.

Besides these tuberculous infiltrations, in which the tubercular matter is intimately associated with inflammatory exudation of a characteristic kind, tubercle has also an habitual relationship to common forms of inflammation.

A grey miliary tubercle, whilst yet semi-transparent, may be surrounded by lung which is quite permeable, crepitant, and to the naked eye healthy. Examined by the microscope, the small bloodvessels around it are somewhat enlarged and clustered, as if they had been obstructed and pushed outwards by the tubercle.† The adjoining vesicles present the appearances already dwelt upon. There is no evidence of inflammation here.

* By blastematous, is here implied only a tendency on the part of the exudation to form induration matter. It may not be quite a correct use of the word.

† See, for illustrations of this, p. 443; and, for the explanation, p. 425, in Jones and Sieveking's Pathological Manual.

A miliary tubercle may become opaque, and still have no inflammation around it; it may also undergo centric softening without inflammation; but as a rule, when softening commences in the tubercle, inflammation around it, if absent hitherto, commences likewise. Perhaps we never find a distinct tubercle softened throughout to complete liquefaction without its being surrounded by a zone of lung affected with inflammation in some shape. What is true of one miliary grey tubercle is equally so of groups. Innumerable distinct grey tubercles may be strewn throughout a lung, or a portion of lung; and for so long as they remain small, distinct, and unsoftened, there may be no inflammation. This does not apply to primary yellow tubercle. We never see them thickly strewn without some kind of attendant pneumonic change. An ancient yellow tubercle, it is true, may be found capsulated by a fibrous envelope, around which the lung is all but healthy. Here there was once a certain amount of inflammation—small in amount and adhesive in type, it may be,—although there is none now.

When grey miliary tubercles are thickly clustered in groups, local chronic inflammation is common around each group, in the form of grey semi-transparent induration. When grey tubercles become entirely opaque, and still more constantly, when they begin to soften, if not already enclosed in a patch of grey induration, they are now surrounded each singly by a circle of red hepatization, the intervening lung remaining crepitant and not congested, or greatly congested but still permeable; or a portion of lung including numerous distinct tubercles may be entirely in a state of red hepatization. Hence, when a number of distinct tubercles are softening, we have co-existing either an equal number of local miniature pneumonias, or else one patch of pneumonia including all the softening tubercles.

As a softening tubercle increases in extent, it never does so at the expense of healthy lung, but always of lung which has previously been the seat of inflammatory exudation. To the ravages of softening lung, therefore, inflammation is quite essential. Our first indication of commencing softening—the fine moist crackle—is due to hypersecretion in the minute bronchial tubes close around the tubercle, which is occasioned by local inflammation.

The process by means of which, out of a series of neighbouring tubercles, a large vomica is formed, is as follows:—The intervening lung inflames, and assumes some form of solidification,—if slowly, the grey induration; if quickly, red hepatization; ordinarily the former. Molecular disintegration and liquefaction of the solidified lung ensue, and so permit the already softened separate tubercles to open into each other, and form one common vomica. As long as the exudation seals up the bronchial tubes, so long does the vomica remain closed. Such a closed vomica, full of pus-like fluid, may be found the size of a walnut. After a time, by the same destructive agency which has enlarged the dimensions of the vomica, a bronchial tube is opened into, and a cavity results.

Where there is a number of minute local pneumonias, there is usually bronchitis in local patches only. During the sleep of miliary grey tubercles there is not, as a rule, any bronchitis or any pleurisy whatever. When the grey tubercles begin to soften, they begin to irritate; they now

become "thorns in the flesh," usually not before. The reactive local inflammations correspond.

Pleurisy, when spontaneous in its origin, whether general or in local patches, usually responds only to the presence of tubercles near to the pleural surface of the lung. If a superficial tubercle is small, not prominent, and is semi-transparent, the pleura over it may remain bright and unaltered. If the tubercle projects, or becomes opaque, the pleura over it is white and thickened. Any irritation beyond this is shown by every possible degree and result of pleurisy, with or without the deposition of tubercle in the adhesions which may be formed.

The bronchial tubes and the pleura near to primary yellow tubercle are never found free from inflammation.

The morbid anatomy of the bronchial tubes in tuberculized lungs may be briefly summed up by saying, that every result of bronchitis is found; the only peculiarity being, that as long as the tubercles allow a large portion of the lung to remain healthy, so long is the bronchial inflammation more or less limited to the vicinity of those tubercles which are pursuing the destructive course. There may be great local destruction of lung without extension of bronchitis beyond the immediate neighbourhood of the part.

In that part of the lung which is free from tubercles we may find, to any extent, congestion, œdema, emphysema (partial), red granulous hepatization, red smooth solidification, or grey hepatization. These possess no peculiarity in their anatomical features dependent on their occurrence in a tuberculous lung.

With non-inflammatory simple blood-exudations the connexion of pulmonary tubercle is small. Pure blood is found in the air vesicles in the rare instance of death during hæmoptysis. Such extravasation of blood, with laceration of lung tissue (pulmonary apoplexy), is very rare. Clots of blood have also been found in cavities. Point-extravasation of red globules interstitially in some part of the lung tissue, or in the tubercles, is never wanting. Of simple fibrinous effusion without inflammation in a tuberculous lung, there is no proof. Passive œdema it would be difficult to distinguish from the common active kind.

Destructive Course of Chronic Pulmonary Tubercle.

The *destructive course* of tubercle comprises degenerative changes in the tubercle itself, inflammatory action in the tissues adjoining, and, commonly, increased deposition of tubercle.

Softening.—After remaining passive, or quietly increasing in magnitude, for an indeterminate period, varying probably from a few weeks to an unknown number of years, tubercle begins to soften. Semi-transparent tubercle first becomes opaque, and remains opaque and firm for an indefinite time. It next loses its firmness, usually first in the centre, which assumes a buff colour, and by degrees softens to the consistence of thick paste. Liquefaction proceeding, the whole tubercle changes into a creamy fluid, and the softening is complete.

During the firm opaque stage of the grey tubercle, in the tubercle cells we discern no other change than that they are no longer as translucent as before; but in their relative proportion to the free molecular matter there is a marked difference. Compound tubercle cells are few. Tubercle



Fig. 17. Grey, semi-transparent tubercle, turning yellowish-white—i.e., opaque-grey tubercle. It presents—

- More free oil molecules;
- More free granules and free tubercle cells;
- Fewer compound cells;
- Fewer nucleated cells than in the semi-transparent stage;
- Still tenacious and tough—not in least cheesy.—(Weale.)

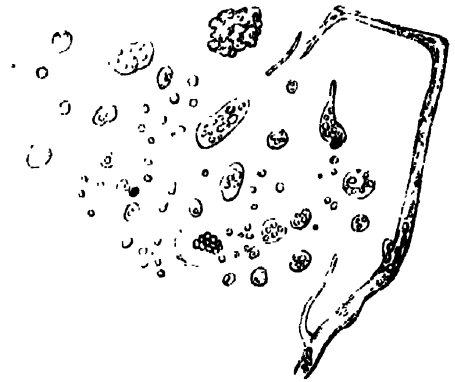


Fig. 18. Opaque-grey tubercle, liquefied. From creamy fluid in a small closed cavity. (Size of a pea.)—(Weale.)

- A few shreds of fibrils;
- A bit of loose bloodvessel with shrivelled red globules in it;
- Free fat dots and granules;
- Various forms of cell, all fatty—a few tubercle corpuscles;
- The whole loose and floating;
- No well-formed pus cells.

corpuscles are also less abundant. Oil molecules abound. The outlines of the air vesicles are imperfectly traceable. The matrix is still tenacious and tough, and the whole moves together under examination. •

In the pulpy stage the matrix has become liquid, the pulmonic fibres are broken into lengths, and molecules are still more abundant.

In the creamy stage, there is simply a larger proportion of fluid. We find shreds of pulmonic fibres; bits of small bloodvessel, either fattily degenerating, or merely shrivelled; various forms of nebulous cells; occasionally large fatty epithelium plates and compound tubercle cells, which have escaped disintegration; oil molecules and granules in abundance; and pus cells or not, according to circumstances. If there be communication with the air by means of an opening into a bronchial tube, pus cells are always found, and make up the bulk of the fluid; but when the vomica is closed, a small one (size of a pea) may not present any pus cells in the pus-like liquor which fills it. • A large one (size of a walnut), though closed, I have invariably found to contain pus cells; and in addition there may be seen in the contained fluid bronchial epithelium, enveloped red blood globules, and pigment cells; bits of lung-fibres; dots of mineral salts, and small blood crystals.

If active inflammation surrounds the vomica, however small this may be, pus will be found in the liquor of the tubercle.

As crude yellow tubercle is opaque from the first, the first indication of softening in it consists in loss of firmness. It first becomes cheesy, then rough and creamy. Softening more rapidly affects the entire tubercle than in the grey variety, but occasionally when a yellow tubercle is large, the centre will be cheesy before the rest. A greater abundance of yellower oil molecules, and of free granules, fewer cells of any kind, excepting tubercle corpuscles, and a more muddy appearance altogether, are the microscopic distinctions. In the liquor obtained from centric softening, whilst it has



Fig. 19. Opaque-grey tubercle, liquefied. From creamy, thick pus-like contents of large closed cavity. (Size of a large walnut.)—(Weale.)

- a.* Granule cells } of all sizes.
b. Pus cells }
c. Bits of fibril. *d.* Mineral dot.
e. Orange blood crystal. *f.* Black pigment.
g. Enveloped blood globules.
h. Free fat and granules abundant.
 No special tubercle cells.

Granule and pus cells and free fat most abundant of all; pus cells did not show the special pus nucleus with acetic acid. Otherwise with water not distinguishable from ordinary pus cells.



Fig. 20. Yellow tubercle, liquefied.

- A.** Creamy liquor from centre of otherwise crude yellow tubercle.—(Ford.)
 No pus; no compound tubercle corpuscles;
 Oil molecules, granules, and small tubercle corpuscles only.
- B.** Puriform liquor of large yellow tubercle, liquefied throughout.—(Ford.)
 Pus cells; granule cells; columnar epithelium; oil molecules; free granules;
 A few single tubercle corpuscles;
 No compound tubercle cells.

not yet implicated the rest of the tubercle, and consequently not yet reached the lung tissue, we find only tubercle corpuscles, oil molecules, granules, and liquid, never pus nor inflammation globules. As soon as the entire tubercle has liquefied, and the softening has thus reached the lung tissue, we find pus and granule cells. (See fig. 20, A and B.)

The whole of a tubercle does not usually soften at once. Softening may commence at the centre; or at the periphery; or at both at once; or when the tubercle is large, it may commence in several points at the same time. The rule is, that when the tubercle is distinctly circumscribed, and no active inflammation exists around it, softening commences at the centre, and may be exceedingly slow in reaching the circumference. But when softening commences at the circumference, it does so in consequence of inflammation in the adjoining lung tissue, and is never long before it involves the entire tubercle. The irregular softening of diffused tubercle is due to the circumferential or centric commencement of softening in each or several of the small tubercles, of which the larger mass is but an aggregation. In every instance that part of the tubercle is first to soften which is first deprived of its means of support:—the part most distant from supply when the centre commences; the part most immediately interfered with by adjoining inflammation when the circumference.

A cavity may be of any size, from that of a pinhead to that of one capable of holding a quart; and may be surrounded by lung in every imaginable condition, excepting that of health. *A thin filmy membrane

may be all that separates it from the lung tissue; or its boundary wall may be a dense white fibrous layer, of very variable thickness. The surrounding lung may be merely congested, or hepatized, or, as is most common, it may be the seat of grey induration. However dense, white, and shining the wall of an old cavity may appear, it is never anything more than imperfect fibrous tissue, and is cartilaginous only in outward appearance.

It is generally stated that cavities enlarge chiefly by the melting down of fresh tubercle deposited in their wall. This is not quite exact. Fresh tubercle is met with either in the shape of distinct nodules, or of grey induration on the outside of the wall of the cavity, but in the actual membraniform wall we do not ordinarily find characteristic tubercle cells. Proceeding from within outwards, we find next to the contained fluid a layer of granule cells and fat molecules, forming a sort of pyogenic pavement epithelium. This rests upon a delicate network of fibrils, amongst the meshes of which more granule cells are freely scattered. The cells for the most part are nucleated. Some are pus cells, but the majority have not exactly that character. A few pigment cells, and blood globules, and glomeruli are sometimes found. Fibre cells, fine and transparent, are largely seen, more and more developed as they are farther from the cavity. When the neighbouring lung is permeable, it is separated from the outside of the fibrous wall by a layer of soft gelatinous exudation, which shades off into the tissue of the lung. More frequently, grey induration is in contact with the fibrous wall of the cavity.

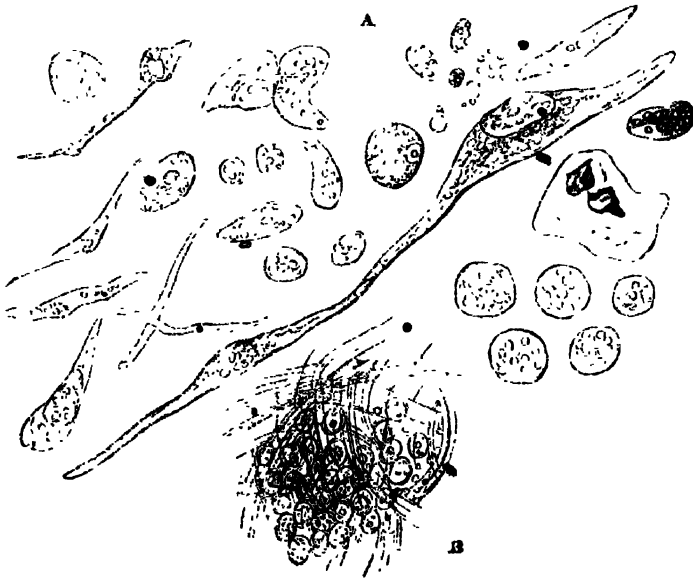


Fig. 21. Pyogenic membrane, lining a closed cavity ($\frac{1}{2}$ inch by 1 inch), grey tubercle predominating.—(Weale).

A. Soft innermost layer, next to liquid contents. + 400.

Made up loosely of nucleated granule cells; fibre cells; pus cells; black and orange pigment cells; blood crystals in filmy envelope; no specific tubercle cells.

B. Basement layer. + 250.

Made up of granule cells and free oil molecules and granules set in the meshes of soft fibrillation membrane.

The contents of an old cavity are little else than pus.

Beneath the soft pyogenic lining of a large cavity I have in two instances found bloodvessels in the state of fatty degeneration. In another instance I found fatty bloodvessels in a portion of distensible lung half an inch distant from any deposit of tubercle, the epithelium of the air vesicles here being fattily degenerate. This makes four cases in which I have now seen fatty degeneration of the small bloodvessels of the lung in phthisis.—Near to, but not mixed up with, tubercle, in one; within grey tubercle, on the enclosed wall of an air vesicle, in one; in the wall of a large cavity, in two.



Fig. 22. Fatty vessels.

- A. a. Bloodvessel in state of fatty degeneration, from wall of closed cavity next to pyogenic membrane. From 2nd case, slight hæmoptoë.
 b. Exuded and clustered red blood globules, appearing as patches of brown and blue pigment, in the relative position to bloodvessel in which they are drawn.
 B. c. Another small bloodvessel, in a state of simple atrophy, with fatty degeneration in places. From 3rd case, slight hæmoptoë.
 d. A heap of blood globules within the vessel.
 C. Small bloodvessel commencing fatty degeneration, epithelium fatty around it, from soft, distensible portion of lung, half-an-inch from grey tubercle. From 4th case, never had any hæmoptoë, except the microscopic kind.

If we trace a large bronchial tube towards the wall of a closed voïnica, its blind extremity is first stopped up by a plug of yellow tenacious concrete pus, and then by firm grey gelatinous matter, into which by degrees the tube seems to be transformed.

Nature of Softening.—The softening of a tough tubercle does not necessarily imply absorption of the solid and deposition of fluid in its place. It is a physical molecular change in the original material of tubercle, such as is exemplified in the centric softening of fibrinous clots, and fibrinous exudations, which has so often been mistaken for pus. Such, also, as

occurs, according to Mr. Paget, when a large firm collection of inflammatory exudation is suddenly converted into the fluid of an abscess. Spontaneous softening, therefore, is not a special characteristic of tubercle, but belongs to it in common with many other firm morbid formations. Just before the fluid stage, when the tubercle is on the eve of liquefying, but is yet only soft and moist, the tubercle cells are larger, plumper, and more opaque than at any previous period. They also now manifest changes of shape under manipulation, as observed by the microscope, which they cannot be made to do at any other time. This larger size and more uniform outline probably indicate physical imbibition rather than growth; impending disintegration rather than development. It shows that the tubercle cells are about to change their condition; that they can no longer remain stationary; that the power which they have hitherto possessed of maintaining their own feeble nutrition is gone. As liquefaction advances, most of the cells disappear, and in their stead we have oil molecules, granular detritus, and liquid. Besides the true oil molecules, highly refracting particles of a peculiar modification of albumen (Dr. Parkes), and particles of crushed phosphate of lime (Dr. Jenner), cannot by the eye alone be easily distinguished from oil molecules. A little micro-chemistry is requisite. The lime effervesces with, or is dissolved by, an acid; the albumen is scarcely altered by ether; the oil molecules are either greasily dissolved away, or else much brightened, by ether. The lime indicates calcareous degeneration; and the albumen in minute particles also indicates degeneration equally with the oil molecules.

Softening of tubercle, then, essentially consists of two combined modes of disintegration—fatty degeneration and liquefaction. The two do not seem in all cases equally to participate. The more there is in the softening of molecular transformation, the slower and safer and more likely to admit of arrest, is the disease. The more there is of liquefaction, the quicker and more destructive is the progress. The former change is more allied to normal disintegration of tissue. The latter to disease. The former admits of slow ulterior and not unsafe transformations. The latter is connected with more inflammation of surrounding tissue.

Destructive Inflammation.—When a tubercle is fully liquefied, the surrounding lung tissue, if not already inflamed, always becomes so; just as the integuments over an enlarged lymphatic gland inflame when this has fully suppurated. But as the skin in most cases does not wait until supuration has been completed, before it begins to inflame, so, in like manner, the pulmonary texture seldom waits for the completion of softening in the tubercle ere it inflames. Inflammation, molecular death, and supuration are the terminal steps in the local destructive course of tubercle; and, when united, by their havoc, so riddle the lungs with channels and cavities, as constantly to excite our wonder that life could have been at all compatible with such an amount of disease.

Increase of Tubercle.—Tubercles are found of obviously different age and condition in almost every microscopic examination of chronic phthisis. The deposition of tubercles is therefore progressive. What causes this progressiveness? In the first place, and mainly, the persistence and increase of the cachexy. Then, inflammation around existing tubercles,

provided the cachexy is great. We have a right to infer this, if only from the fact that tuberculization, in the shape of tubercular infiltration (which might be called tubercular inflammation), is more extensive, more rapid and destructive, in the later stages of chronic phthisis, when the general cachexy has attained its worst. And lastly, it is possible that the mere presence of tubercle may serve to some extent as an attractive focus for more.

As tubercle corpuscles possess no power of growth after they have once attained their ordinary small size, which they probably do at once, and no power whatever of self-multiplication by cell-reproduction, a tubercle can only increase in size (as already remarked) by the accretion of fresh material from without. Now, does the existence of some tubercle in the lung exert a strong attractive influence for more? Whilst the diathesis remains unimproved, does deposition of tubercle, after it has once commenced, go on fast in proportion to the number of separate deposits already laid down?

That a tubercle is to some extent a centre of aggregation is certain, or it would never increase at all. But it is less clear that this aggregation results from any such attraction of like-for-like as that which operates, for example, in crystallization. A tubercle, however small, obstructs a number of capillary vessels, produces a stasis in the circulation, which, so long as inflammation has not obliterated the vessels, is most considerable close to the tubercle. Exudation is consequently favoured here more than elsewhere, and provided the blood plasma be tuberculous, and in proportion as it is so, will such exudation tend to increase the quantity of tubercle. At the same time, we cannot prove that there is none of the like-for-like attraction (homogeneous affinity) between the elements of a completed tubercle and the blood plasma; but there are reasons for considering that if any such do operate, its influence cannot be strong. If it were strong, the dissemination of small tubercles would be the exception, and not the rule. And, unless we assume the first deposition to have exhausted the tubercle material for a time, existing tubercles would continue to increase in size in a geometrical ratio; tubercles would scarcely be laid down at all in fresh situations; and temporary lulls of the disease would be greatly less common than they are. There is another reason for inferring that tubercle has not any *strong* tendency to invite the formation of more. Such attraction in the living economy generally proceeds from, and depends upon, active vital changes, in which active cells draw from the blood what they require for growth and multiplication. In this way cancer acts as a centre of attraction, for its cells grow, multiply, and for a while remain as cancer matter. But not so in tubercle. Here, cell life is at its minimum; development is small; multiplication null. Material is not wanted for growth, for after the first there is no growth; and material is therefore not attracted. Still tubercles do enlarge; and if a tubercle does in some way promote further deposition around itself, of what moment is the question—whether it does this by means of direct attraction exercised upon blood plasma, or merely by mechanical obstruction at the spot favouring exudation? The question has this importance and interest. In the one case—Tubercle must ever keep up Tuberculosis, for Tubercle makes Tubercle. In the other, it is the tuberculous diathesis alone which occasions increase of tubercle, and

the tubercle already deposited has not, from the mere fact of its presence, at all events before the period of softening, any direct influence in keeping up the tuberculous diathesis. In the former case, the mere existence of a tubercle would offer an all but insuperable obstacle to correcting the diathesis. In the latter, could we correct the diathesis, the existing tubercle would not of necessity cause more to be laid down.

These remarks apply only to tubercle when completely formed. Whilst forming, the compound tubercle cells which line the air vesicles probably do attract their plasma from the blood; and, therefore, at this stage of unformed but forming tubercle, the spot of lung affected must certainly be considered as a focus of attraction for tubercle; but this ceases as soon as the given air vesicles are crammed with exudation, and the tubercle has thus become completed.

Progressive increase in the quantity of tubercle is the principal element in the destructive course of chronic phthisis, not only because it proves that the cachexy still continues in active operation, that "the snake is not even scotched," but also because the larger the tubercles become, the more certain are they to soften quickly (and therefore dangerously); for the farther the oldest portions of each tubercle are pushed inwards towards the centre of the deposit, the farther are they removed from the source of nutritious supply, and from the influence of living tissue.

We have so far been engaged with the development and destructive course of Tubercle. We shall have another opportunity of completing our attempt to describe the Natural History of this morbid deposit, by considering the changes which attend its arrest, and which constitute the Conservative course, or so-called Curative changes, of Chronic Pulmonary Tubercle.

(To be concluded.)

ART. II.

On the Morbid Appearances in Death by Cold. By FRANCIS OGSTON, M.D.,
Aberdeen.

THE extent of our practical information relative to the effects of intense cold on the human body is but very limited. Though instances of death from this cause are less infrequent, even in this country, than is generally supposed, very few inspections of such bodies are known to have been undertaken, and those which have been recorded by medical writers are brief and defective. In these circumstances, it may be useful to give a detailed account of a few cases of this sort which have been seen and examined by the writer.

The three first are recent cases, the parties having perished in the vicinity of Aberdeen during the severe snow-storm of last spring.

CASE I.—W. M., a male, aged 17, of weak mind and wandering habits, was known to have passed the night of Saturday, the 3rd of February, 1855, in a turnip shed. On the Monday following, after sunset, he came to a farm-house in the parish of Newhills, about five miles from his usual place of residence, where he had some oaten bread and milk presented to him, of which he partook freely, and left the house, having been refused his request of a night's lodging. About an hour and a half subsequent to this he was found lying on the snow, not far from the farm-house, in an

insensible state, and foaming at the mouth. He was scantily clad in ragged clothes, which were wet at the time. Without delay he was carried to an outhouse and laid upon some loose straw, where he calmly expired in about three quarters of an hour. Deceased had never been affected with epilepsy.

The body, inspected thirty-eight hours after death, well formed and well nourished. The head nowise deficient in size or shape. The lips and portions of the cheeks of a florid red. The rest of the surface, including the dependent parts of the trunk and limbs, unusually pale. The countenance presenting a smiling aspect. Joints rigid. *Pupils dilated. Scrotum corrugated. Scalp, cranium, membranes, and substance of the brain, all unusually pale and bloodless. A marked degree of the same pallor and bloodlessness of the mouth, throat, and air passages. A little frothy mucus in the trachea. Lungs collapsed, and containing less blood than usual. Right cavities of the heart, and both venæ cavæ, enormously distended with a continuous fibrinous mass, surrounded with a thin layer of watery blood. Blood, in a clotted state, completely filling the left cavities of the heart. Liver congested: the blood in this viscus, as well as that in the heart, approaching more to the colour of arterial than that of venous blood. Stomach, intestines, and urinary bladder unusually pale and bloodless. Farinaceous food and milk-curd in the stomach. Spine healthy.

The only structural changes encountered in the cavities of the body were, dryness and considerable firmness of the brain, flattening of its convolutions; slight old adhesions of the lungs to the walls of the chest; partial emphysema of these organs; and buff-coloured patches on the surfaces of the kidneys. The urine, which was in some quantity in the bladder, was free from albumen.

CASE II.—M. M^d. F., a female pauper, aged 70, of intemperate habits, was accustomed to pass the night in a barn at Cruibstone, in the parish of Newhills. *Between nine and ten on the evening of the 13th of February last (1855), she was seen not far from, and walking in the direction of, the barn. About seven o'clock on the following morning she was found dead on the snow close to the door of the building.

On the 15th, at 1 P.M., the body was inspected, when the following appearances presented themselves:—A blush of bright redness on the front of both knees. The same redness, but less bright, over both cheeks, and the lower border of the right wrist. Lips and finger-nails bluish. Rest of the surface, including the dependent parts of the body, pale. Joints rigid. Pupils moderately dilated. Tip of the tongue in contact with the front teeth. Veins on the outer and figured surfaces of the brain full of blood. Interior of the brain containing more blood than usual. A quantity of frothy mucus in the throat. Right cavities of the heart unnaturally distended, and containing a large fibrinous clot and a quantity of fluid blood. A very large quantity of blood, partly clotted, in the left cavities of the heart. Both venæ cavæ, and the aorta and pulmonary artery, distended with fluid blood; which, like that in the heart, except when viewed in mass, was of a much brighter red, than usual. Lungs partially collapsed, and containing only a moderate quantity of blood. Liver gorged with fluid blood, of the same appearance as that in the heart.

Minute injection of the capillaries of the peritoneal coat of the smaller intestines generally, giving them a uniform rose hue.

The structural changes in the body were as under:—A minute patch of fatty degeneration in the coats of the basilar artery. Attenuation, to a moderate extent, of the right ventricle of the heart. Slight thickening of the tricuspid, and of two of the aortic valves. Atheromatous patches in the coats of the ascending aorta. Portions of the liver cirrhused. Melanotic oval or rounded deposits, under the mucous coat of the stomach. Cortical portions of the kidneys attenuated, and of a buff colour. A little milky urine in the bladder, becoming more opaque by heat.

CASE III.—B. A., or F., or R., a female, 83 years of age, of intemperate habits, was seen on the 28th of February last (1855), at Cotton, going northwards. On the morning of the 9th of March her body was found on the snow, at the side of a footpath through a field, from three to four miles to the westward of Cotton. Her clothes were soaked in water, and it was conjectured that her body had been covered with snow, and only exposed to view on its melting by the thaw, which had commenced a few days previously. She was lying in a crouching attitude.

The body examined on the same day (9th):—Joints flaccid; lips, and instep of the right foot, of a bright-red colour; dusky redness of the ears, forehead, and upper part of the face; rest of the surface, including the dependent parts of the body, pale; countenance placid; pupils moderately dilated; soles of the feet blanched and plaited; scalp bloody; a thin layer of clotted blood, an inch and a quarter in greatest breadth, under the integuments of the forehead, to the right of the mesial line; a thin layer of clotted blood on the surface of the anterior lobe of the right hemisphere of the brain, at its right side and back part; two small clots of blood, immediately below the surface of the brain, at the same part; brain generally, firm; tongue retracted; frothy mucus at the root of the tongue, and in the larynx; lungs pale, and containing but little blood; right cavities of the heart, and the vessels connected with them, distended, and containing blood, partially clotted, and a large fibrinous clot, which also filled the trunk and larger branches of the pulmonary artery; left cavities of the heart, and the bloodvessels connected with them, containing an unusually large quantity of blood, partially clotted, and a tenacious fibrinous clot, which was traced as far as the descending aorta and half way up the common carotid arteries; the blood in the heart and large bloodvessels, except when viewed in mass, appeared almost as bright-hued as arterial blood; a little glairy mucus in the stomach; spleen pale and shrivelled; a moderate quantity of bright-hued blood in the liver; spine healthy.

Besides the above, the following appearances were met with in the cavities of the body:—Cerebral arteries mostly everywhere dilated, thickened, and inelastic, but otherwise natural; heart large, and its left ventricle hypertrophied; aorta, and pulmonary, subclavian, and carotid arteries, in the same condition as the cerebral; mitral and tricuspid valves thickened and traversed by firm cartilaginous bands; portions of both the large lobes of the liver cirrhused; kidneys mottled with yellow patches, their cortices attenuated, and the urine in the bladder albuminous.

CASE IV.—On January 2nd, 1837, J. G., aged 60, a street porter, had been engaged in delivering goods from an early hour till between six and seven o'clock in the evening. Returning from the Bridge of Don (about two miles from Aberdeen), and feeling wearied, he sat down on the bank of the Aberdeenshire Canal at Nelson-street. The night being very frosty, he could not resist the tendency to sleep. After passing some time in this state, he awoke confused, and forgetting where he was, he advanced in the direction of the lights in the town, and fell into a lock in the canal, up to the neck in the water, then covered with a thick crust of ice. Some men passing, hearing his cries for assistance, drew him out of the lock, and conveyed him to a hovel, without fire, in the vicinity. At this place he had a glass of spirits, and after resting for a little, and relating the above particulars, the men undertook to convey him to his own house. He accordingly set out with them, in his wet clothes, although cold and numbed, and proceeded with difficulty about a hundred yards, when he became insensible. After carrying him about four hundred yards further, and being refused admittance into different houses, he was taken into a shop, when it was found that he was dead. This was at half-past nine, P.M. A few minutes later he was seen by an assistant, who found his lips, nails, and general surface pale, the pupils dilated, and the limbs very cold, with only a little heat remaining at the precordia.

The body was examined forty-one hours after death. Prominences of the elbows bright-red; red patches, not so bright, on the fronts of the thighs, and on the right shin; lips, and remainder of the front of the body, very pale; dependent parts of the head, trunk, and extremities reddish, the colour of the trunk approaching to lividity; scalp bloodless; a moderate quantity of blood in the back part of the longitudinal, and in the lateral sinuses; membranes and surface of the brain bloodless; a considerable number of bloody points in the interior of the brain; epiglottis of a uniform bright-red; slight redness of the back parts of the larynx and trachea; blood, of a purplish hue, filling the cavities of the heart on both sides, the venæ cavæ, the subclavian veins, and the aorta and pulmonary artery throughout their whole course—the blood, with the exception of some clots in the inferior cava, in a fluid state; frothy mucus in the air cells of the lungs; liver, spleen, kidneys, the left lung, and the dependent parts of the right lung, all moderately congested with fluid blood, of the same appearance as that in the heart and large vessels; food in the stomach; bladder full of clear urine.

The brain was of a firm consistence throughout. The lining membrane of the lateral ventricles, at their foreparts, and that of the third ventricle, was thickened, and of almost cartilaginous firmness. There were some old adhesions of the left lung to the chest and diaphragm. With these exceptions no structural changes were discoverable in the cavities of the body, or about the spine.

Remarks.—These cases present so many points in common, and several of these of so peculiar a kind, that little room is left for hesitation in assuming that the cause of death had been the same in all. Without, however, formally enumerating the particulars in which they coincide, it may be sufficient to direct attention to a few of the more prominent of these.

One striking feature in all the cases was the colour of the blood in the heart and elsewhere, so different from that which presents itself in ordinary inspections of the dead body. In one of the cases this was so marked that the appearances exhibited by the thoracic and abdominal cavities, when laid open, suggested the comparison of them to those of a living animal.

Another marked peculiarity, disclosed by all the cases, was the amount of blood accumulated within the cavities of the heart on both sides, and in the large vessels connected with them, arterial as well as venous. In three of the cases large fibrinous clots had separated from the blood both in the heart and bloodvessels.

Corresponding with this accumulation of blood in and around the central organ of the circulation, was its marked absence or deficiency in other parts of the body. Thus the general surface in all was pale, and the usual suffusions wanting in the dependent parts of the head and trunk. In three of the cases the scalp was pale and bloodless. In the first case the same paleness and bloodlessness was observable in the skull and its contents. In the fourth case, though there was blood in moderate quantity in the longitudinal and lateral sinuses, with a considerable number of bloody points in the interior of the brain, the surface of the brain and its membranes were pale and bloodless. In the second case, while the veins on the outer and figured surfaces of the brain were full of blood, the sinuses were empty, and the membranes natural. In the third case the sparing effusions of clotted blood at the corresponding parts of the scalp, and of the surface and interior of the right hemisphere of the brain, pointed to a fall about the time of the woman's death; a conjecture favoured by the circumstances, that she had been seen previously in drink, and that the footpath beside which her body was found was bordered by a deep trench, filled with snow to the same level. In the first case the mouth, throat, and air passages were bloodless. In the three first cases the lungs contained less blood than usual. In Case 1 the stomach, intestines, and bladder are noted as unusually pale and bloodless.

Another peculiarity in the above bodies is the existence in all of diffused patches of bright redness of the surface, at various parts of the front of the face and limbs; and perhaps also the minute injection of the exterior of the smaller intestines in Case 2, and the mucous froth in the air passages in three of them, and in the air cells of the lungs in the remaining instance, may be regarded in the same light.

The paucity of details in the few published cases of death by cold renders it impossible to institute any complete or satisfactory comparison between these and the cases just adduced. From the notices collected by Dr. Copland* we gather that Quelmalz found "the large veins and arteries filled with polypous concretions;" and Cappel, "the blood and fluids accumulated chiefly in the pectoral and abdominal viscera." Dr. Kellie again, in the two cases he has published,† met with the same injected appearance in the intestinal tube which was encountered in Case 2. On the other hand, Quelmalz, Rosen, and Kellie speak of cerebral congestion; to a greater or less extent, in their inspections.

In these circumstances, and till we have a larger collection of cases

* Dictionary of Medicine, vol. i. p. 357.

† Transactions of the Medico-Chirurgical Society, Edinburgh, vol. i. p. 84.

before us, it would be unsafe to draw any positive conclusions as to the immediate cause of death from cold, whether from syncope, as the foregoing cases would suggest, or from coma, as contended for by Dr. Kellie* and others. With the view of throwing some further light on this obscure point in pathology, I shall subjoin a few instances of the deaths of infants from exposure, in which it appeared to me that cold had played the principal part in leading to their fatal issues. In regard to these I shall only remark, that while they will be seen to exhibit several phenomena in common with those observed in the bodies of the adults as given above, it will be noticed that they show differences in other respects; a result which, assuming that the cause of death was the same in all, may have in part been owing to the less intensity of the cold to which the infants were exposed, and in part to peculiarities of constitution at their early age.

CASE V.—J. B., a male infant of six weeks, was exposed, thinly clad, during a cold night in the beginning of June, 1827, to a journey in an open vehicle. At first the child cried a good deal, and refused to suck, then lay quiet, and at the end of the journey was found to be dead.

When inspected next day the following appearances were noted:—Front of the chest, sides of the belly, and back part of the pelvis, of a deep-red colour. Scalp natural. Dura mater minutely injected. Veins on the surface of the brain and in the choroid plexuses turgid. A number of bloody points in the interior of the brain. Tip of the tongue protruded beyond the gums in front. Reddish frothy mucus in the trachea and bronchi. Minute injection of the inner face of the sternum, the surface of the right lung, the exterior of the aorta, and of the pulmonary artery at their origin, the omentum, the outer coat of the stomach and of the whole of the peritoneal surface of the intestinal tube throughout its course, but brightest over the smaller intestine. The blood in the injected vessels of a bright hue. Cavities of the heart gorged with blood—colour not noted.

CASE VI.—A male infant, seven hours after birth, was laid down alive in a lane in Aberdeen, about 8 o'clock, p.m., on August the 24th, 1833. It was discovered shortly after apparently dead, its limbs cold, and only a little heat remaining at the precordia. It was merely wrapped in a piece of dark cotton cloth. Thinking the child was in a fit, the person who found it had it immediately placed in a warm bath, when a medical gentleman, called in, pronounced it to have been some time dead.

Inspection, forty hours after its discovery.—Trunk and limbs rose-hued, approaching to scarlet on the extremities, and to lividity on the back; lips, points of the fingers, soles of the feet, and nails, all livid; joints rigid; face anxious; pupils dilated; tip of the tongue fixed between the gums in front; scalp bloodless; pia mater injected; brain natural; serum in the upper part of the spinal canal; epiglottis of a uniform bright-red; diffuse dull-redness of the interior of the trachea and bronchi; lungs of a scarlet hue; dark fluid blood filling the right cavities of the heart, the left auricle, the coronary veins, the venæ cavæ, the subclavian and jugular

* Dr. Kellie's inspections are unfortunately incomplete. Nothing is said, in his narrative, of the state of the heart; and it is at least doubtful if the chest was opened at all, in either of his cases.

veins, and the aorta throughout its whole course; frothy fluid in the air cells of the lungs; blood, in some quantity, in the lungs, partly of a florid hue, but chiefly of the usual venous appearance; liver and kidneys congested with dark fluid blood; bladder empty.

CASE VII.—A female infant, five days old, had been exposed alive on the banks of the Dee, near Aberdeen, on January 18th, 1841. It was found dead on the following morning.

Inspected on the morning of the 20th. Face reddish; surface of the neck, lips, and vulva also reddish; lips and nails livid; rest of the surface bloodless; joints rigid; pupils natural; tongue protruded beyond the gums in front, and its tip livid; glairy mucus at the lower part of the trachea; pericardium, thymus, surfaces of the heart, and of the aorta and pulmonary artery, minutely injected; cardiac veins turgid; cavities of the heart on both sides, the venæ cavæ, and the aorta and pulmonary artery, distended with dark blood, mostly in a clotted state; lungs collapsed, bright red externally, and very much loaded with dark fluid blood; liver of a deep purple hue; liver, spleen, pancreas, and kidneys much congested with dark fluid blood; surface of the pancreas injected; intestines minutely injected; mesenteric and gastric veins turgid with blood; bladder empty; moderate injection of the sheath of the spinal cord; scalp vascular; pia mater minutely injected; veins on the surface of the brain turgid; interior of the brain mottled with red patches.

CASE VIII.—A female infant was discovered after some days' exposure in a deserted quarry-hole at Bourtie, in February, 1849, where it had been laid down alive.

The following particulars are borrowed from the notes of the inspection by Dr. James Jamieson, now of Edinburgh.* Except slight redness of the dependent parts of the trunk and extremities, the rest of the surface, including the lips, pale and bloodless; portions of the surfaces of both lungs vermilion-hued, their interior much congested with fluid blood; blood of the same character in both sides of the heart, but in greatest quantity in its right cavities; scalp, cerebral membranes and sinuses, and the surface and interior of the brain, all remarkably pale and bloodless, as were the muscles of the face, neck, and arms.

ART. III.

Critical Examination of the Evidence for and against the presence of Epithelium in the Air Cells of the Human Lung. By GEORGE RAINEY, M.R.C.S., Lecturer and Demonstrator of Practical and Microscopical Anatomy at St. Thomas's Hospital.

SCARCELY was the existence of a layer of nucleated cells (called "epithelium") on the free surface of all the internal membranes, whether situated within the cavities of the body, where they exist in the form of shut sacks, or for

* This experienced medical jurist was engaged with me in the investigation of the three adults examined in February and March (Cases 1, 2, 3).

the dermis, discovered, and its presence in such situations considered as constant and universal, before the opinions of anatomists became divided as to the correctness of this inference, its existence in particular parts of certain organs being by many observers altogether denied. Thus, whilst no doubt whatever was entertained of the general presence of the newly-discovered structure in the tubuli uriniferi of the kidney, some observers denied its existence on the Malpighian tufts. Also on the membrane lining the articular cavities, where it encloses capillary plexuses called synovial glands or fringes, the presence of a distinct epithelium was allowed by all; whilst on the non-vascular parts of the same membrane, especially those which are by some supposed to cover the articular cartilages, no such structure was by many considered to exist. In certain parts of the lungs also, as the lining membrane of the bronchial tubes, the epithelium, being particularly apparent, was universally admitted without any hesitation, yet its existence in the air cells was, and still continues to be, a point much disputed by anatomists and physiologists. This marked contrariety of opinion upon points which at first sight appear so easy of demonstration as the mere existence in certain parts of structures so characteristic as epithelia are generally described to be, can scarcely fail to strike with astonishment those who do not employ the microscope, and, in their opinion, to detract greatly from its utility as an instrument of research. But it is apparent also from these statements that there is a bright as well as a dark side to this subject; for they show that although anatomists differ widely in their opinions about some points, they perfectly agree upon others. Microscopical observers do not express any doubt as to the fact of the existence of epithelium in the uriniferous tubes, on the synovial fringes, or in the bronchial tubes; nor do even their descriptions of these epithelia, though they are possessed of different characters in these several parts, materially differ. And this positive knowledge could never have been attained had not these facts been revealed by the microscope, so that upon some of the most important points of minute anatomy there is obviously sufficient uniformity in the opinions of those who have employed this instrument in their investigations to convince every person whose mind is not fettered by prejudice, that its aid ought not to be refused or all its indications to be discredited because there are points upon which anatomists are not yet unanimous. On the contrary, these diversities of opinion upon the same subjects only show that there is something more or less directly connected with these disputed points, which has not been perfectly apprehended, and that they require more minute and extensive examination than they have yet received in order that they may in their turn be raised from the misty regions of uncertainty, where they are liable to an indefinite number of interpretations, according to the individual bias of those who contemplate them, to that state of absolute certainty when they will appear in too clear a light to allow of being any longer made subjects of discussion. Thus considered, it will be seen that these discrepancies, in the place of being put down to the score of the inapplicability of the instrument, are chargeable on those who use it, or who do not use it with sufficient care.

Although I have alluded in these general observations to three instances in which the opinions of anatomists are at variance concerning the

existence or non-existence of epithelium, I shall confine my observations entirely to the part of the dispute which has reference to the air-cells.

Now, whilst on the one hand the mere circumstance of a pavement epithelium having been described by several anatomists in the air-cells of the human lung furnishes an amount of evidence of its existence, which, if unanimsly confirmed by all future observers, would be regarded as conclusive; so, on the other hand, the fact of its existence being denied by anatomists who entertain no doubt whatever of the presence of epithelium in the smallest bronchial tubes, may fairly be adduced as negative evidence in favour of its non-existence; and as the strength of this evidence rests upon a variety of incidental circumstances, such as the want of agreement in the accounts of those who have described the epithelium in question, the absence of those characters, both positive and negative, which distinguish the same class of structures in other parts of the body, the general accuracy of the descriptions, &c. &c., all these points have a right to be fairly and fully considered. But the strongest argument against the presence of epithelium in the air-cells, the one upon which I chiefly dwell, is based upon the fact that the cause of the epithelium-like appearance which they present under the microscope admits of being accounted for independently of any supposition implying the existence of a lining of epithelial cells. Those which I consider as the best and only satisfactory proofs of the presence of an epithelium, are derived from the facts of its admitting of demonstration on the same part both when *in situ* and when detached. In the latter case, the true character of its cells can be best determined, and in the former, its situation and relations; but it is only the combined evidence of both which will show whether the appearances which any part presents, are due to an independent structure, or whether they are an integral part of the tissue upon which the epithelium is supposed to be placed.

From Kölliker's account of the epithelium lining the air-cells of the human lungs, it would appear that there is great difficulty in demonstrating it *in situ*, in consequence of the remarkable readiness with which it is said to be detached from the cell-wall; and as this difficulty was not found to exist in animals, in which the lungs were examined a shorter period after death, it is, I presume, attributable to the extraordinarily rapid post-mortem changes of which this epithelium is thought to be so very susceptible in the human body. Dr. Andrew Clark informs me "that the lung ought to be removed from the body as soon after death as possible within five hours," in order that it may be in the most fit state for demonstrating the epithelium of the air-cells. Now, according to these statements, the investigation of this epithelium in the human subject seems to be so hedged in with difficulties, that the opportunities of examining it must be very rare, for it is not often that post-mortem examinations are allowed so short a period after death; consequently there is the greater probability that those who have described this structure in the human air-cells have fallen into that class of errors which arise from mistaking that which is merely accidental for that which is constant; and in this instance it seems much more probable that such a mistake should have been made than that there should exist in the human lungs a structure endowed with such an anomalous property. I am perfectly

aware that very rapid post-mortem changes do take place in some of the more concealed and delicate epithelia—such is particularly the case with that lining the capsule of the lens—but that this should be so pre-eminently the case in one which is always exposed to the contact of the atmosphere, is most improbable, especially as it is not a property of the epithelium lining the smallest bronchial tubes, which in all respects would be similarly circumstanced. Besides, the pavement epithelia in other parts of the body are not of this evanescent character; they can all be demonstrated in the ordinary post-mortem subjects, both with ease and certainty. The cells of this epithelium are said by this author to repose immediately upon the fibrous layer of the membrane forming the air-cells; which is another anomaly, as the cells, of most other epithelia are situated upon a thin membranous film, devoid of fibres, well known by the name of “basement membrane.”

There is another anomalous character belonging to this epithelium, especially insisted upon by those who have described it, which is the distinctness said to be acquired by its cells when they are in a state of disease. It is said* that the epithelium of the air-cells is normally shed, though not readily detected in health, yet that it is easily discovered in disease. This, I may observe, is the reverse of what is observable in this class of epithelia in other parts of the body, in which, on the contrary, their specific characters become obscured and masked by disease. So that in those parts on which pavement epithelium is well known to exist, it is always sought for when they are in the healthiest state possible; and the more free they are from disease, the more distinct and better marked are the characters of their epithelia. Besides, the mere fact that a structure, whose presence in its normal condition is so remarkably uncertain as to be demonstrable only by a kind of accident, should only acquire that degree of distinctness which is necessary to render it recognisable when the membrane upon which it is placed is diseased, is certain evidence that at least some part of the change which it has undergone to give it its distinctness is the effect of abnormal causes, and it is a strong argument in favour of the entire appearance being due to the same agency. Such evidence as this is more in favour of its being an abnormal product than a healthy structure. This supposition agrees with appearances which I have myself observed in diseased lungs. In some of the apparently healthy air-cells of the human lungs, situated near to those in which tuberculous deposit existed, I have sometimes found a glassy-looking substance, mixed with large globules of oil, which might be mistaken for epithelium. This, when I first heard of Dr. Thomas Williams's hyaline epithelium in the air-cells, I thought might possibly have been something similar to that which he gives this name. This however, when closely examined, I found to be devoid of the characters of epithelium, and also, on referring to Dr. Williams's paper, that it does not agree at all with the newly-named structure which he designates Hyaline Epithelium. The want of agreement in the statements of those who have described the epithelium of the air-cells, is so striking, that it is impossible that all who have intended to write about this epithelium can have had precisely the same structure in view, and have applied the same term to the same appear-

* See Cyclopædia of Anatomy and Physiology, part xlv, p. 271.

ance. Kölliker, notwithstanding the great difficulty he complained of in meeting with epithelium in the healthy human lung still adherent to the pulmonary membrane, makes no mention of want of distinctness or completeness of its individual cells; but, on the contrary, in these respects the cells must have been remarkably perfect to have admitted of the measurement of their breadth and thickness, and to have even allowed of their exact distance from the capillaries over which they are situated being estimated, which is stated to be about half of the thickness of the membrane. Dr. C. Radclyffe Hall, on the contrary, makes no allusion to the difficulty or uncertainty of finding this epithelium in the human lung, but describes its cells as wanting that distinctness of outline and regularity of form which characterize other pavement epithelia. The epithelium of the air-cells in man, according to Dr. Hall, has less sharply-defined outlipes than most other varieties of pavement epithelium; but the flat cells are bounded by a dim line of limitation. In appearance they are thin, almost transparent, and have a slightly nebulous, somewhat ill-defined nucleus, very different from the bright, sharply-cut nucleus of the pavement epithelium of the mouth, for instance. In size and shape these cells vary greatly. They are pentagonal, hexagonal, or polygonal, with angles more or less acute or rounded, according to their mutual fitting into each other. Their general character is, that they constitute a fine but dimly-defined pavement epithelium of a single layer. Now, if the attention of these two observers had been directed to the same structure, it is difficult to understand how the deficiency of distinctness, and want of definition of the alleged epithelial cells, and more especially their extreme irregularity in form and size, mentioned by the one, should have escaped the notice of the other, particularly as the attention of the latter, in taking the actual dimensions of these cells, could not have failed to have been arrested by the negative peculiarities described by the former. As such an oversight could scarcely have been possible, there is every reason to conclude that the epithelium mentioned by Kölliker is not the same as that described by Dr. C. Radclyffe Hall.

Dr. Thomas Williams, author of the article, *Organs of Respiration*, in the 'Cyclopædia of Anatomy and Physiology' for March, 1855, observes, "That the nuclei and granules of the epithelium of the air-cells are less declared than those of any other description of epithelium;" and therefore, in consideration of these negative characters, he proposes to distinguish it by the term "hyaline epithelium." Dr. T. Williams does not describe his hyaline epithelium minutely, but adopts the views of Van der Kolk, from whose work he introduces a plate, showing the hyaline pavement epithelium which lines the interior of the air-cells. The representation of this imaginary epithelium agrees very well with the description given of it by Dr. Hall, especially in respect to the extreme irregularity in the form and size of the epithelial-cells; but their outline and nuclei are represented with more distinctness than Dr. Hall's description would justify. However, I have no doubt but that the description of the one and the delineation of the other apply to the same structure.

* Sometimes there is a greater degree of exactitude and precision in the description of microscopical structures than their extreme minuteness or intrinsic deficiency in positive anatomical characters, with our present

means of examination, render attainable. This practice, though inspiring great confidence with some, is always a source of suspicion with those who have had much experience in the use of the microscope, and who, consequently, know the difficulties attending the examination of minute parts with this instrument.

Both Dr. Thomas Williams and Dr. R. Hall describe the epithelium of the air-cells as forming only a single layer. It is observed by the former, that "they (the epithelial cells) are adjusted accurately, as a single layer, edge to edge." As this statement implies the utmost degree of distinctness in these cells, and conveys an idea that they admit of being easily examined, and their relations to one another and to the structures with which they are connected accurately determined, it is completely at variance with the accounts which they themselves have given. For if these cells really are as deficient in outline and as irregular in form and size as described by one of these authors—or if they possess a degree of deliquescency which renders an examination of them whilst *in situ* in the perfectly healthy human lung a very uncommon occurrence, as stated by Kölliker—and lastly, if to these characters be added a degree of tenuity and transparency likening them to glass, and thus requiring them to be distinguished from other pavement epithelia by the term hyaline, or vitreous, as has appeared necessary by Dr. T. Williams, I do not see how such a degree of precision and exactitude, under circumstances so unfavourable, could possibly have been ensured. Besides, if it be a fact, as intimated by Dr. R. Hall, that different growths of these cells exist together, the statement that there is only one layer cannot be true; for if we suppose that on each side of the very thin membrane between adjacent air-cells it is only the middle-aged corpuscles which, by their "mutual fitting into each other," form the single epithelial layer in question (and this supposition is by no means inconsistent with the alleged facts, as it is only cells of a similar form and size which would admit of the accurate adjustment of their edges mentioned by Dr. Williams), there is no room for such cells as are advancing to a state of maturity, or for those which, having passed the middle period, are declining into a state of decay, and therefore there must be more than one layer of epithelial cells of some sort or other. This involves a contradiction which I must leave for others to reconcile. The statements of Kölliker respecting the distance of the alleged epithelium of the air-cells from the capillary plexuses in their walls, implies a degree of regularity and uniformity in the structure and position of these parts which have no existence in nature, and therefore leave the accuracy of his measurements, his plates, and his description of the epithelial cells, open to suspicion. Such is the negative evidence, or rather the want of consistent positive proof, of the existence of a pavement epithelium in the air-cells of the human lung.

I will now consider the cause of the epithelium-like appearance presented by the air-cells which has led to the belief that they are lined with epithelium. But first, in justice to myself, I must correct a misrepresentation of my views upon this question occurring in Dr. Radclyffe Hall's paper,* entitled, *The Mode of Development of Tubercle in the Lung in Chronic Phthisis, its connexion with fatty degeneration of the epithelium of the*

* British and Foreign Medico-Chirurgical Review, No. xxx. p. 477.

air-cells, &c. &c., in which he says, "That the existence of an epithelium at all in the air-vesicles has been questioned by physiologists of high repute, but is now very generally admitted. BLOOD CORPUSCLES, seen through the walls of the capillaries, were indicated by Mr. Rainey as having possibly been mistaken for epithelial cells." I wish to observe, that no such statement ever proceeded from me, or that I ever entertained so improbable an opinion. Dr. Hall can scarcely have seen what I had written upon this point, or he would not have so completely misunderstood my meaning.

But before proceeding to show to what the epithelium-like appearance, which has led to the belief in the existence of an epithelium in the air-cells, is due, I must again advert to the want of agreement which is observable in the descriptions of those who have written upon this structure. The microscopic appearances which are described by Kölliker are so dissimilar to those by Van der Kolk, that they cannot apply in any respect to the same corpuscles. Van der Kolk found the employment of acetic acid necessary to give the nuclei of the corpuscles which he has described their utmost distinctness. Kölliker, on the contrary, does not state that any chemical substance was employed for that purpose; and if these corpuscles, as seen by this observer, were sufficiently distinct to admit of the precisest measurement, and if, at the same time, they were so loosely connected with the fibrous membrane with which they are in contact, it may be fairly inferred that he did not use acetic acid, as under such circumstances it would have been of more injury than benefit. But there is a still greater want of agreement in the delineations of this epithelium by these two authors, although they are both professedly intended to represent the same structure. I may observe, that Van der Kolk's plate, exhibiting the hyaline pavement epithelium lining the interior of the air-cells, is a good but slightly exaggerated representation of an appearance which I have myself frequently seen; but that Kölliker's figure, showing a thin section of a few air-cells from the human lung, with the epithelium projecting from the pulmonary membrane, has no resemblance to Van der Kolk's, nor is it in the least like any microscopic appearance in the human lung which has ever occurred to me. I cannot help thinking that the corpuscles described so precisely by Kölliker were some of the imperfectly-developed epithelial cells from the smallest bronchial tubes, which had been detached in the process of manipulating, and had got by accident into the air-cells. This is so common an occurrence, that such corpuscles are generally found in greater or less quantities in these cells, but they have not the most distant resemblance to pavement epithelium, as seen in other parts of the body, nor to the imaginary hyaline pavement epithelium represented by Van der Kolk, as copied by Dr. Thomas Williams in the 'Cyclopædia of Anatomy and Physiology.' As to the appearances represented in Van der Kolk's plate, and the greater part of those described by Dr. R. Hall, which in most respects agree with the latter, I am satisfied that they are produced almost entirely by the capillary network in the walls of the air-cells. This appearance I noticed several years ago, and at that time convinced myself that it was not due to the presence of epithelium, as can be seen by my papers *On the Lungs*, published in the 'Medico-Chirurgical Transactions' of 1845 and 1848; but as this still

continues to be a point which is much disputed, I have lately repeated the examination of these parts, and the result has served only to confirm my former convictions of the accuracy of the opinion which I then expressed.

Since the period alluded to, the question has been brought, by the published results, either in the form of descriptions or plates, of several anatomists who have paid great attention to this subject, into a much narrower compass; its decision will consequently be more easy. Dr. C. Radclyffe Hall's description of an epithelium in the air-cells of the human lung, Van der Kolk's plates, and Dr. Thomas Williams's observations, and some preparations which Dr. Andrew Clark has lately had the kindness to lend me, showing what he considers to be an epithelium in the air-cells of the human lung, and what I had myself observed both before and after I had become acquainted with the labours of these observers, has convinced me that we are perfectly agreed as to the actual existence of the same microscopic appearances in the air-cells of the human lung, and therefore that the only point to be decided is whether this appearance is due to the presence of a pavement epithelium in these parts, or to the cause I before mentioned. The method which I have employed to determine this point has been to compare, side by side, perfectly fresh specimens of lung with those in which the vessels had been filled with colouring matter; by this mode of procedure, there is no difficulty in recognising many of the capillaries in the uninjected, or imperfectly injected, parts, and in distinguishing their course, to a greater or less extent, along the membrane which connects them together. This is especially easy in those capillaries whose arched inosculations can be seen projecting beyond the circular free border of the pulmonary membrane, where it forms the openings of communication between the air-cells, in which capillaries the "slightly noduleous ill-defined nuclei," as described by Dr. R. Hall, can be seen.

That these nuclei belong to the walls of the projecting capillaries, and that they are identical with those irregularly dispersed through the membrane, where the capillaries are so blended with its tissue as not to present a distinct outline, as figured by Van der Kolk, is so evident as to admit of no doubt whatever, at least in my mind. That these nuclei are confined to the bloodvessels is further proved by the fact that none of them are visible in injected specimens in the areolæ of the capillary plexuses, where nothing but the pulmonary membrane is present, whilst in the same specimens the epithelial cells are perfectly distinct in the smallest bronchial tubes. The capillary plexuses in the external wall of the peripheral air-cells, having the largest meshes, are best adapted for this examination. However, as this fact might be attributed to the improbable and perfectly gratuitous supposition of a much greater liability to rapid post-mortem changes in the epithelium of the air-cells than in that of the ultimate bronchial tubes, I have particularly examined this point in frogs which have been scarcely dead, in which the same facts and appearances are obvious. In these reptiles, the ciliated epithelium does not extend over the capillaries which are directly concerned in the process of respiration, but is confined to the folds situated between the lateral sacculi or pouches, where the cilia can be seen beautifully in action; whilst in the pouches, which correspond to the air-cells of the mammal, no epithelium of any kind can be seen, but only the

persistent nuclei of the capillaries of the air-sacculi. These nuclei are present also in the wall of the capillaries upon which the ciliated epithelium is situated, and are visible when it is removed. The latter capillaries are of a different size, and have a very different arrangement from those in the sacculi: this is especially remarkable in the female frog.

The next thing which may be mentioned as contributing to the epithelium-like aspect presented by the air-cells, is the obscure retiform appearance produced by the irregularly oval spaces, corresponding to the meshes of the capillary plexuses, and bounded in the recent lung by the vessels containing more or less of the fluid part of the blood. Considering the remarkable regularity in the shape and size of the epithelial cells or scales composing a layer of true pavement epithelium, and the distinct outline which they ordinarily present, it would appear impossible that such appearances could ever so far assimilate to pavement epithelium as to admit of being mistaken for it. I consider that such a mistake could not be made if the real characters of this epithelium were kept in view; but it must be observed that Dr. R. Hall's description of the appearances which he takes for an epithelium in the air-cells, is so distinguished by its negative characters, which he is under no temptation to exaggerate, that they would serve much better to prove the converse of his position, than to establish the position itself. The most unepithelial-like part of the appearance referred to in his description, is that which alludes to the shape and size of the epithelial cells, from which they may much more easily be imagined to resemble the capillary network, with its irregularly oval meshes, as they are obscurely seen in the walls of the air-cells, than any well-marked pavement epithelium which has ever been described. In this respect Van der Kolk, judging from his plate of the epithelium lining the air-cells, seems to me to have fallen into the same error.

The sharp threads of elastic tissue are the last structure to be noticed as having a part in producing the confused epithelium-like appearance in the air-cells. They are especially distinct and defined in the lungs of quadrupeds. In these animals also, especially in the younger ones, as also in children, the persistent nuclei in the capillaries of the air-cells are particularly distinct, much more so than in the adult human subject; hence probably the reason why Kölliker had so little difficulty in finding what he considered to be epithelium in the lungs of these animals; and Dr. Andrew Clark, in speaking of the lungs of children, observes "that he never fails to find in the child's lung a lining epithelium to the air-cells." I wish particularly to observe that it is not to any one of these structures, taken separately, that I attribute the mistake in question, but to the appearance produced by the whole taken together; although I believe that the persistent nuclei in the coat of the capillaries have the largest share in causing it, and next to that, but in conjunction with it, the imperfectly-developed epithelial cells which had been detached from the terminal bronchial tubes, and got into the air-cells during the manipulation. The oval meshes of the capillary network will also contribute their part, and serve, as before stated, to give to these imaginary epithelial cells their characteristic irregularity of form and size, and to furnish the "dim line of limitation" by which they are surrounded.

Having now shown that there is quite sufficient in the healthy textures

of the human lung, when seen under favourable circumstances, to account for that amount of resemblance to epithelium which has led to the belief of its existence in the air-cells, I shall conclude the discussion of this subject by adducing the evidence furnished by comparative anatomy against this opinion. This I do not consider to be necessary, from a conviction that the facts and arguments which have been advanced have not been sufficient to establish this point, but the deductions from this source being, in my opinion, so strong and legitimate, and even of themselves sufficient to decide this question against the unsatisfactory evidence on the opposite side, could not with propriety be omitted in this communication. In birds, the air, after having passed through the bronchial tubes, which are lined by ciliated epithelium, as in mammals, is not received, as in man, into comparatively large irregularly-shapen cavities formed by a distinct fibrous membrane, but it enters minute irregular passages, which soon lose themselves in the meshes of an extremely dense plexus of capillaries, into which this air freely passes, becoming, as it were, extravasated between and around the vessels. These meshes, the analogues of the air-cells of mammals, and the air-sacculi of reptiles, are so exquisitely minute, especially in those birds whose habits require the highest state of development of their pulmonary organs, as not to equal in size the smallest epithelial cell; and, therefore, they cannot contain a lining of epithelium, or even a basement membrane, without its encroaching so much upon their capacity as totally to prevent the admission of air, and thus to render them altogether incompetent to the performance of their function, and to cause their structure to resemble liver more than lung. And hence the lungs in this class of animals furnishes an incontrovertible proof that an epithelium is not required in those parts of a respiratory organ in which the process of respiration is carried on, and therefore that it is not an indispensable part of the air-cells. But this kind of negative evidence is not confined to the lungs of birds; there are many mammals in which the respiratory powers are much more energetic than in man, whose air-cells are too minute to admit of a lining of pavement, or any other kind of epithelium recognised by anatomists, without unfitting them for the performance of their function. To complete this evidence, and to show how simple the means are, and how few the conditions necessary for a perfect organ of respiration, I have only to notice the breathing apparatus in insects. In this class of living beings many of the tracheæ are so remarkably minute as scarcely to be distinguishable by the highest and best magnifying powers. In these instances there can be no room for pavement epitholium, either on the surface of these tubes in contact with the air, or on that which is said to be bathed in their blood. It will be seen, then, from all that has been advanced, that as epithelium is not an essential constituent of the air-cells or interstices of birds and some mammals, it is no abuse of analogical evidence to infer that it does not exist in the air-cells of man, especially as the proofs to the contrary have been shown to rest upon appearances in the air-cells perfectly unconnected with the presence of epithelium, and which admit of easy and rational explanation.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON FORENSIC MEDICINE & TOXICOLOGY.

By BENJAMIN W. RICHARDSON, M.D.

Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Forensic Medicine at the Grosvenor-place Medical School.

I. TOXICOLOGY.

Fatal Poisoning by Nitrate of Potassa.—Dr. JOHN SNOWDEN relates the case of a German, who spoke English perfectly, who went into a store and asked for “bitter salt,” meaning sulphate of magnesia. The attendant supposed he meant saltpetre, and gave him half a pound. The man took three ounces and a half at one dose. His bowels were opened three times within three or four hours. He complained of a slight sense of heat in the epigastrium, and drank a good deal of water. About five hours after having taken the saltpetre, he suddenly fell out of his chair and died. There was no post-mortem examination. The *rigor mortis* was very imperfect; the lips were of almost a natural pink hue, and the appearance of the countenance was so life-like, that some persons who were present doubted the propriety of interment on the third day.—*New Jersey Medical Reporter, and Philadelphia Medical Examiner*, April, 1855.

Nitrate of potash not infrequently poisons cattle when given to them as medicine by injudicious persons. Mr. Truckle has reported in the ‘*Veterinarian*’ for March, that in two cows out of three to which a fluid of a pound of saltpetre was given, two died within three hours, and apparently without a struggle. The post-mortem examination showed that the third and fourth stomachs were inflamed, especially the latter. The editor of the ‘*Veterinarian*’ remarks on this subject, that he was consulted some years since respecting the cause of the deaths of several cows, and on inquiry he found that their owner had been giving them a half-pound dose of Armenian saltpetre (nitrate of soda), which he had bought as a cheap purgative. The same writer remembers to have seen three heifers poisoned by taking three quarters of a pound of nitre each. They all died within twelve hours. The agent had been sold by a grocer in mistake for Epsom salts.

Post-Mortem Appearances in Poisoning with Phosphorus.—Dr. REISIG relates the post-mortem appearances observed in a man and his wife, poisoned with lucifer matches. Both patients died in four days. The similarity in the appearances observed is remarkable.

THERESIA SCHOBESBERGER.

External Appearances.—Slight loss of hair, as in poisoning with arsenic. Mucous membrane of mouth very pale.

Internal Appearances. (a) Abdomen.

1. The external surface of the stomach was red; the vessels, at the small cur-

JACOB SCHOBESBERGER.

External Appearances.—Slight loss of hair. Oral cavity pale. Face and breast jaundiced.

Internal Appearances. (a) Abdomen.

1. The same appearances were observed, except the dark spot.

vature of the stomach were distended, and filled with dark-brown blood. At the posterior surface was a roundish dark spot, five inches long by two wide.

2. On opening the stomach there was a strong smell of phosphorus. The whole mucous membrane of the stomach was covered with a brownish-black colour, and covered with a thick layer of mucus. At the cardia, base, and lesser curvature were many streaky dark-red spots. The mucous membrane was remarkably soft and loosened; the muscular coat was red.

3. The mucous membrane of the upper third of the small intestine presented the same appearances as that of the stomach; it was covered with a thick brownish mucus. There was no smell of phosphorus.

4. The liver was of a remarkable yellowish-white colour on the surface and on section: it was void of blood. The gall-bladder was filled with a large quantity of dark-green bile. The urinary bladder contained dark-brown urine.

(b) *Thorax*.—The lungs were very dark in colour, and contained much blood. The heart was soft, pale, and empty. Each pleura contained five ounces of red fluid.

2. On opening the stomach, there was a very faint smell of phosphorus. The mucous membrane of the stomach was slightly reddened, covered in several places with dark-brown spots, and so soft that it could be easily removed by the nail from the muscular coat. The mucous membrane was nowhere corroded.

3. The same appearances were noticed. The peritoneum and mesentery were everywhere covered with numerous extravasations, as were also the abdominal muscles.

4. The liver was of a light colour, and void of blood. The gall-bladder and urinary bladder presented the same appearances as in the other case.

(b) *Thorax*.—The lungs were very dark-red, congested, and soft. The pleura and pectoral muscles presented numerous dark-red spots of extravasation. Each pleura contained four ounces of sanguineous fluid.

Wochenschrift der Gesellschaft der Aerzte zu Wien, No. 20, 1855.

Albumen and Hydrated Magnesia as Antidotes in Poisoning with Corrosive Sublimate.—L. SCHREDER has performed a number of experiments on dogs and rabbits, with regard to the use of albumen and hydrated magnesia as antidotes in poisoning with corrosive sublimate, from which he arrives at the following results:

1. Albumen is not to be depended on as an antidote. The albuminate which is formed is soluble, not only in an excess of albumen, but also in such albuminous substances as may be present in the stomach and intestines, and is especially liable to be taken up by the acids with which it may there meet.

2. Albumen can only be useful when given so as to produce vomiting, or where vomiting is excited by tickling the throat.

3. Hydrated magnesia cannot be regarded as an antidote, as it forms from the corrosive sublimate an oxide of mercury, which is itself poisonous.—*Deutsche Klinik*, 1854, No. 8; and *Prager Vierteljahrschrift*, 1855, Erster Band.

Case of Poisoning by German Sausages.—Mr. W. H. MICHAEL, of Swansea, relates the following case:—On March 22nd, 1855, Mr. Michael was desired to see a child living in Postern-lane, Swansea. Upon arriving at the house, he found a fine little boy, between four and five years of age, lying on his grandmother's lap. The mother had been given the evening previously a German sausage, of which the eldest son had partaken at once. This had made him ill through the night; vomiting and purging had taken place to a considerable extent. The little boy now ill had eaten some of the sausage (according to the statement of the mother, only one or two very thin slices) for breakfast, about two or three hours before Mr.

Michael arrived, at two o'clock p.m. Shortly afterwards he had vomited. About half an hour before he was seen, convulsions had come on; he had also been violently purged. When seen, the general surface was cold; the limbs rigid; the teeth very firmly clenched; the pupils largely dilated, and insensible to stimulus; and he had occasional convulsive spasm of the lips. The lips were livid; the face was deadly pale; no pulse could be felt at the wrist; and the respirations were only three in the minute. He died in about ten or fifteen minutes, and about three hours after eating the sausage, as nearly as could be learnt from the confused statements of the mother.

The remaining portion of the sausage, which was one of the German smoked and dried kind, showed some incipient softening and decomposition (not putrefactive) at the surface; the interior both looked and smelt good. Careful analysis detected no traces of metallic poison. The mouldiness frequently spoken of by authors could not be seen.

The post-mortem examination showed the stomach half full of pieces of sausage, floating in a pulpy mass, half digested, of the same. Considerable irritation and manmillation of the mucous coat existed, especially towards the pyloric orifice. The mucous coat of the small intestines was irritated throughout, small puncta of blood being observable over the surface, which was bathed in increased mucous secretion. The brain was congested, as were also the thoracic organs. The other portions of the body (which, although well formed, was much attenuated) were healthy.

In Germany such cases are fearfully prevalent. In Wurtemberg alone, according to official returns, more than four hundred cases have occurred in the past fifty years, of which a hundred and fifty died. Of these, forty per cent. occurred in the month of April; and this has been put down as a matter of some importance in determining the character of the poison, which is said usually to manifest its symptoms in from twenty-four to forty-eight hours after ingestion. Recent researches appear to have proved, contrary to what has long been supposed, that unprepared meats, far advanced in the putrefactive process, or belonging to diseased animals, may be often eaten with impunity. Mr. Michael inclines to the doctrine lately put forth by M. Van der Court, that the poison of sausages may be due to the development of an elementary vegetable—the *sarcina botulina*.—*Association Medical Journal*, August 17, 1855.

Poisoning by Cantharides.—On the 27th of October, at two A.M., Dr. C. H. HILDRETH, of Gloucester, Massachusetts, was called to a patient who, early on the preceding evening, purchased from an apothecary about half an ounce of a powder supposed to be the pulvis aloes cum canellâ, commonly called *pikery*.

The medicine was delivered by a boy in attendance. The patient put the powder into a bottle, added to it a tablespoonful of gin, and took two spoonfuls of the mixture—the usual dose for the irritation caused by ascarides. He slept well until twelve o'clock, when he woke with severe pain in the lower part of the abdomen, extending into the lumbar region, but most intense just above the pubes. This rapidly increased, and in two hours became almost unendurable. There was some nausea, but no pain anywhere except as above mentioned. On examination, the mixture, the supposed pikery, proved to be powdered cantharides. Free vomiting was at once produced by sulphate of zinc and free draughts of warm water. The powdered flies were expelled at each act of vomiting; but the pain in the abdomen was not in the least relieved. Large injections of warm water were frequently administered, and ten grains of camphor, with one grain of sulphate of morphia, were given every half-hour, until four doses had been taken. Great relief resulted. Three hours afterwards, the patient passed water freely; his urine was natural, and without any trace of blood. He had suffered from priapism to a painful extent for a short time, but it had entirely subsided. The patient was sitting up; the pain was very slight, and did not return. He had suffered no inconvenience from the large dose of morphia. Four days later, he had pain

in all his joints, especially the knees; his eyes were inflamed and painful; slight effusion was apparent in the knee-joints; and there was some inflammation of the sclerotic, which yielded to simple remedies, or, more probably, subsided spontaneously. His perspiration emitted a strong cantharidine odour, especially in the axillæ. Ten days afterwards he was able to resume his work. • •

The points of interest noted in the report of this case, are—

1. The length of time (about four hours) which elapsed before any perceptible effect was produced by the cantharides. Is not this analogous to the results of its external application?

2. The apparent want of action upon the stomach.

3. The large quantity of morphia taken without producing narcotism. This, however, is often observed in painful diseases of all kinds.

The quantity and quality of the cantharides taken could not be accurately estimated. The patient had taken only a slight supper, consisting of a cup of tea and a piece of bread.—*Boston Medical and Surgical Journal, and Philadelphia Medical Examiner*, April, 1855.

Fatal Poisoning of Fire Persons by Tincture of Colchicum.—On Dec. 7th, 1851, M. JULES ROUX prescribed sixty grammes (somewhat more than fifteen drachms) of quinine wine to five patients, named Steinger, Rougier, Ahmet-Ben-Salah, Gandillon, and Paradis. Four of these patients had undergone severe surgical operations, and were progressing favourably. One had tubercular disease of the testicle. They had all for some time been taking quinine wine, which was administered habitually in the presence of M. Roux during his visits. They were patients in the hospital of the convict prison of Toulon. On the present occasion, by a deplorable mistake, sixty grammes of tincture of colchicum were administered to each patient at eight A.M., in lieu of the quinine. No ill effects were at first felt, and some even said, in a low voice, that the wine was better than that which they had had before. In two hours, two patients were seized with heat in the epigastrium, colic, and vomiting. The surgeon on guard soon saw them, and found the five patients pale and cold, with small pulse, intense griping, nausea, vomiting, and abundant and frequent evacuations from the bowels. Warm water, tannin, and coffee were administered; sinapisms were applied to the limbs, and warmth to the surface of the body. M. Roux saw these patients at half-past two P.M., when the following symptoms were presented:—Paleness of the skin, general coldness, circulation very slow, pulse very small, and in two of the cases quite imperceptible; burning pain in the pharynx and œsophagus, unquenchable thirst, great heat of surface, intolerable pain in the epigastrium and entire abdomen, repeated vomiting, and numerous serous, yellowish stools, but without mucosity or streaks of blood. Intellect and speech, sensation and motion, remained entire. In one patient alone there had been, since the morning, some troublesome noise in the left ear. Nothing remarkable was observed in the state of the pupils. At this time, M. Roux could not discover what the patients had taken; but as the symptoms were evidently those of a narcotico acrid poison, he prescribed mucilaginous and albuminous mixtures and injections, and ordered emollient poultices to be applied to the abdomen. A consultation was then held, when it was concluded that the poisoning arose from tincture of colchicum, and a draught of infusion of balm, sulphuric ether, laudanum, and syrup was ordered to be given every quarter of an hour. The poultices, mucilaginous drinks, and calorific means were continued. At five P.M. the symptoms remained the same in four patients. In Gandillon the vomiting and purging had ceased, the skin was warm and moist, and the pulse was raised. In spite of constant care, three patients died during the night—Ahmet-Ben-Salah and Gandillon at a quarter past three, Paradis at a quarter past four. At six A.M. the two surviving patients presented the following symptoms:—Burning pain in the throat, violent thirst, gripings, tenesmus of the rectum and bladder, pains in the loins and limbs, heaviness of the head, sense of oppression, coldness of the skin, lividity of the lips and nails. The

vomiting had diminished; cramps of the legs were observed in Steinger, whom it was necessary to catheterise, on account of retention of urine. Rougier died at ten o'clock A.M., and Steinger at half-past one in the afternoon.

Thirty-six hours after the death of the latter patient, a post-mortem examination of all the patients was made. The temperature of the air was 68° Fahr. The appearances presented in all the bodies were so much alike, that the same description will serve for all.

The expression of the countenance was calm, the eyelids were half open, the pupils were normal; there was blueness of the nails, hands, and some parts of the skin; the skin was generally discoloured, but nowhere corrugated; *rigor mortis* was moderate. There was no ulceration, nor traces of ulceration, in the pharynx and œsophagus; the stomach and intestines contained a little gas and much turbid fluid; the mucous membrane was generally much softened—it was not ulcerated, but was red at different points. The liver was much congested; the gall-bladder contained a moderate quantity of bile; the spleen was gorged with blood; the kidneys were congested; the bladder contained a little urine, and its mucous membrane presented red spots of small extent. The heart was flabby, and contained a little black blood, and small clots of the same colour; the *venæ portæ* and inferior cava were distended; the blood everywhere had the appearance and consistence of currant jelly. The lungs were healthy and crepitant, and free from gravitative congestion: there was no effusion in the serous cavities. The brain and spinal cord were much injected; the cerebral spinal membranes were very red; the cephalo-rachidian and sub-arachnoidean fluids were small in quantity, as was also the fluid in the ventricles; some drops of blood exuded from the cut surface of the brain; there was general softening of the cerebro-spinal axis. The muscles surrounding the splanchnic cavities were of a remarkable deep-red colour throughout; the flesh was firm, and only showed slight traces of decomposition three days after death. Chemical analysis of the fluids evacuated and found in the bodies detected the presence of colchicum.

M. Roux, in his remarks on these cases, believes, with the Italian school of toxicologists, that colchicum poisons more by its depressing action than by producing inflammation of the intestinal canal. He founds his opinion on the following facts:

1. In his patients the poison did not commence to act until two hours after it had been taken into the stomach, and then only when it had been absorbed.
2. The depression of the powers was primary, and not secondary to the irritation of the digestive canal.
3. The traces of inflammation of the intestinal mucous membrane were not sufficient to account for death; but the profound alterations of the nervous system were perfectly in relation with the fatal result.

An inquiry was instituted on these cases by the Maritime Prefect of Toulon, and the apothecary by whom the mistake was made was acquitted by a majority of six votes against two. He was, however, removed from his post; but some months afterwards was reinstated. He has since died.

Not many years ago, and in one of the hospitals of Paris, seven patients died almost at the same moment, by taking, by mistake, a preparation of hydrocyanic acid.—*L'Union Médicale*, March 27th, 1855.

Poisoning by Aconite.—Bappoo Kishmura, a Hindoo priest, aged 50, was admitted into the Janisetjee Jejeebhoy Hospital, in the afternoon of the 4th of August, 1854, under the care of J. PEET, Esq., Assistant-Surgeon. For twenty-four days he had been taking, daily, fifteen grains of a native drug called "bishnak" (a root of the *aconitum ferox*) as a remedy for leprosy, from which he had suffered for several years. Until the morning of the day of his admission he had been using the black variety of the drug, but at the recommendation of a friend he substituted the white variety. The dose was fifteen grains, and was taken in the morning, at ten o'clock. He soon afterwards began to feel uneasy, had a

disagreeable burning sensation in the mouth and fauces, a sense of formication, and some confusion of mind. A friend recommended milk, of which he drank a large quantity. Soon afterwards he vomited freely; and about an hour after the vomiting came, and about four hours from the taking of the drug, he was brought to the hospital. He was then sensible, had a stupid expression of the face, and walked with an unsteady gait, like a drunken person. He complained of heat and burning in the throat, some confusion of mind, and of a sense of formication over the surface generally. There was almost constant vomiting; pulse feeble and rapid; skin coldish, and covered with moisture; pupils natural. A sinapism was applied to the epigastrium, and stimulants of ammonia were administered. The vomiting continued till twelve o'clock. Ultimately the animal warmth returned, the pulse rose, the vomiting ceased. He went home in the course of the day.—*Transactions of the Medical and Physical Society of Bombay, 1855.*

Poisoning by Opium.—A man, of spare habit, and about 25 years of age, was brought to the Bhooj Dispensary by his friends, who stated that he had taken opium for the purpose of poisoning himself. He complained of giddiness and drowsiness. His pupils were contracted, and insensible to light, pulse small and frequent, perspiration profuse. He was treated by Mr. SADASHIEW HEMRAJ, Sub-Assistant-Surgeon, by an emetic of twenty-five grains of sulphate of zinc, but it did not operate. After this, the stomach-pump tube was introduced, and some tepid fluid thrown in and removed, but no opium could be detected; but it smelt of assa-fetida, which was given to him before he was brought to the dispensary. He was made to walk about between two men; but in about an hour stupor came on to such an extent, that he could not be made to move, when he was placed in a chair, and electro-galvanic shocks were passed along the spine and back of the neck. Under this he improved and became alert, when he was again made to walk between two men. In about a quarter of an hour stupor again came on, and the electro-galvanic shocks were repeated. In about three hours he was much better, there remaining slight drowsiness only. After recovery, he said that he had taken about three drachms of opium, four hours before he was brought to the dispensary, and that he had not taken any food for sixteen hours before that.—*Transactions of the Medical and Physical Society of Bombay, 1855.*

Poisoning with Mushrooms.—Dr. MASCHKA relates seven cases of poisoning by mushrooms, which occurred in Prague, in September, 1854. The following symptoms were produced:

CASES 1 & 2.—W. M., a boy, aged 7; A. K., a woman, aged 70. In three hours after partaking of a dish of mushrooms, violent vomiting and diarrhoea, of a yellowish-white fluid, set in; the skin became pale and cool, but without cyanosis; the face pale and sunken; the eyes sunken and lustreless; the nose and mouth were dry. Vomiting left a burning sensation in the throat, and the diarrhoea was attended with crampy pains, extending to the lower limbs. The abdomen was not distended or tender; the urine was scanty; the radial pulse could not be felt; the carotid pulse was weak and very rapid; the cerebral functions seemed unimpaired; and the voice was not altered. In spite of treatment with emetics, oleaginous mixtures, and emollient drinks containing vegetable acids, death occurred in both cases in from seventeen to eighteen hours after eating the mushrooms.

CASES 3 & 4.—J. K., a boy, aged 7, and K. J., a boy, aged 10, were attacked, on the night of September 8th; after eating boiled mushrooms in the evening, with violent pain in the abdomen, vomiting, and diarrhoea. They were admitted into hospital at six A.M. on the 9th. The patient J. K. lay motionless, stiff, and speechless; the temperature of the head was raised; the pupils were dilated; there was spasmodic contraction of the jaws, and occasional grinding of the teeth; the skin was cold, and cyanotic in the lower extremities; the upper limbs were convulsed; the abdomen was in a state of meteorismus; there was no vomiting;

involuntary evacuations of fæcal matter occurred; the pulse was very frequent and thready. An emetic, cutaneous frictions with spirits, and a bath, were prescribed; but the patient died twelve or thirteen hours after taking the poison. [The fate of the patient K. J. does not appear.]

CASES 5 & 6.—K. C., a girl, aged 9, and J. C., a woman, aged 36, ate mushrooms at midday on September 7th, and were attacked in the evening with vomiting and diarrhœa. They were admitted into hospital on September 8th. They both complained of severe pain in the head and abdomen; the head was hot; the pupils were dilated; the speech was stammering and very low; the abdomen tender; the skin cool, and the lower extremities cyanotic. Vomiting had ceased; but there were frequent fæcal evacuations. The pulse was very small and frequent, and the strength was much reduced. K. C. was also very restless, and had convulsive movements of the limbs. Baths were used, and extract of opium with alum was given; but the diarrhœa continued throughout the 9th. In the morning of the 10th, sopor and stertorous respiration set in, and the patients died, with convulsive movements of the extremities, from sixty-six to sixty-eight hours after eating the mushrooms.

CASE 7.—M. H., a man, aged 36, was seized in three or four hours with vomiting, diarrhœa, and pain in the abdomen. The symptoms which he presented were similar to those already related; and he died in sixteen or seventeen hours.

Post-mortem examinations of all these cases were made, and the *ensemble* of the appearances found is thus described by Dr. Maschka:—(a.) There was an entire absence of cadaveric rigidity. (b.) The pupils were dilated. (c.) The blood was generally of a dark-brown colour, and fluid, and had mixed with it here and there small, spongy, dirty yellow fibrinous clots, which were easily broken down by the finger. (d.) There were numerous ecchymoses and sanguineous effusions both in the serous membranes and in the parenchymatous organs. (e.) The bladder was excessively distended with urine.

Dr. Maschka observes that there was the greatest similarity between the symptoms in his cases and those which have been described by other writers as attending poisoning by mushrooms.

From a consideration of the remarks of Orfila and Krombholtz, and from some experiments which he performed on rabbits, Dr. Maschka concludes, with regard to the post-mortem appearances, "that entire absence of cadaveric rigidity, fluidity and dark colour of the blood, distension of the urinary bladder from paralysis, and numerous ecchymoses and effusions of blood in the serous cavities, and even in the internal organs, are appearances, which are as a rule observed in the bodies of those who have been poisoned with mushrooms."

Are the symptoms and post-mortem appearances which have been described above peculiar to cases of poisoning by mushrooms? Orfila found absence of cadaveric rigidity, mobility of all the limbs, and dilatation of the pupils to be the results of several poisons. He also observed, after poisoning with chloroform, ecchymoses in the pleuræ and lungs; dark blue spots on the lower borders of the lungs after poisoning with belladonna, atropine, hyoseyamine, and conium maculatum; and ecchymoses of the lungs, with a dark brown spot on the upper surface of the liver, after poisoning with cicuta. James found small ecchymoses in the endocardium and muscular structure of the heart after poisoning with arsenic; and Gaspard observed the same appearances in animals and man after corrosive sublimate; while Dr. Maschka found sanguineous effusion in the pleuræ after suffocation with carbonic acid gas. But, so far as is known to Dr. Maschka, no poison is followed by so copious extravasation of blood as was observed in his cases; and he hence infers, that the poison of mushrooms has a peculiar influence in producing fluidity of the blood, and consequent extravasation. Whether this be really the case, and whether the extravasations, when there is no evidence, chemical or other, of the presence of a poison, can be considered as certainly, or even probably, warranting the suspicion of poisoning with mushrooms, can only be determined by further researches.

With regard to the chemical detection of the poisonous principle of mushrooms, Dr. Maschka's examinations of the contents of the stomachs of the poisoned individuals add nothing to our knowledge of the subject. He was unable to discover any trace of the poison, either by optical or by chemical tests.—*Prager Vierteljahrschrift*, 1855, Band ii.

Poisoning by Nux Vomica.—Mr. DE CRESPIGNY relates the following case:—A Mahratta, admitted at noon of 31st December, 1853, said he had taken four seeds of nux vomica, each weighing three grains, powdered, in order to cure an ulcer on the back. He complained of giddiness, trembling of the limbs, and spasms, and walked with difficulty; he had not vomited, nor was there any inclination to do so. The pulse was natural, the skin warm, and the pupils natural. Two doses, each containing a scruple, of sulphate of zinc were given, but without producing vomiting. Two pints of water were injected into the stomach, and a feather was passed into the fauces, without avail; four pints of lukewarm water were then injected into the stomach, which produced copious vomiting; the injection was repeated in a quarter of an hour. The spasms increased in severity, and opisthotonos came on. There was temporary relief after the vomiting; but the slightest movement immediately produced spasms. The senses were not affected, though the speech was rapid. Vinegar and water were now given, and repeated every ten or fifteen minutes. Soon after the second dose, two strong purgative enemata were administered, without effect. After a third enema had been given, the bowels were relieved; he began to feel easier, and the spasms were separated by longer intervals. The spasms, which were referred to the cervical region, continued till midnight; the cramps extended upwards from the lower extremities; and the diaphragm seemed affected. He complained at one time of numbness of the lower extremities; but there was no loss of sensation. There was great giddiness throughout, and excruciating shooting pains through the head. The latest effects observed were occasional twitchings of the tendons, and a feeling of incipient spasm. He was discharged on the third day.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

Poisonous Nature of Tobacco packed in Leaden Cases.—The *Union Médicale* for September, 1854, contains some remarks on this subject. The moisture contained in the tobacco oxidizes the lead, and forms a soluble salt. The tobacco becomes covered with a layer of acetate, carbonate, chloride, and sulphate of lead, amounting to from six to thirty grains in half a pound. Tin-foil has therefore, in France, been ordered to be used instead of lead. The presence of lead in tobacco is detected by burning the tobacco, treating the ash with dilute hydrochloric acid, evaporating, and treating the re-dissolved residuum with iodide of potassium, sulphate of soda, and sulphuretted hydrogen.

On a Substance produced near Aden, said to be used by the Somalies to Poison their Arrows.—Dr. ARNOTT had forwarded to him a watery extract, prepared from the root of a tree called "Wabie," a toxicodendron from the Somali country. The tree grows to the height of twenty feet. The poison is obtained by boiling the root in water until it attains the consistency of an inspissated juice. When cool, the barb of the arrow is anointed with the juice, which is regarded as a virulent poison. Dr. Arnott was informed that death generally took place within an hour. He could not, however, ascertain the quantities made use of by the Somalies. In some experiments on animals, in which the poison was administered by the stomach and by inoculation, which were conducted by Drs. Arnott and Haines, the fact seemed to be established that the poison was of no great activity. When death took place, the symptoms more resembled those produced by nux vomica than by any other agent. No apparent drowsiness; spasms slight at first, beginning in the neck, increasing in intensity, extending over the whole body, and finally stopping respiration, and with it the action of the heart. The experiments of Dr.

Arnott, as regards the effects of the poison when swallowed, differ from those of Dr. Haines. A dog treated by Dr. Arnott died in an hour from a dose of five grains; but two dogs treated by Dr. Haines suffered nothing for two hours after taking the poison. At the end of this time they were shot. A difference in the strength of the extracts seems to account for these peculiarities. When death took place, the post-mortem signs were collapse of the lungs and distension of the cavities of the heart, without any signs of inflammation.—*Transactions of the Medical and Physical Society of Bombay, 1855.*

Swallowing Pounded Glass.—An artist, aged 25 years, was admitted into the Jansetjee Jejeebhoy Hospital on the 30th of August, 1852, in charge of a policeman, who stated that the man was seized committing a robbery, and either on his way to jail or soon afterwards broke up and swallowed a quantity of glass of a bottle. The remainder of the bottle was produced—a reddish-brown quart bottle, such as Rhenish wine is imported in. The man complained of a little pricking sensation in the throat and stomach; no febrile symptoms were present. Emetics (of sulphate of zinc) were given, but did not cause the return of any of the glass. Later in the day one ounce of castor oil was given, which opened the bowels three times. The stools were not bloody, but in the bottom of the vessel were found a number of pieces of thin bottle-glass, precisely corresponding with the remains of the bottle shown by the policeman. About twenty fragments were thus passed, the largest about an inch long by a quarter of an inch broad; the smallest as large as a grain of rice. There was great tenderness of the epigastrium, so that leeches had to be applied, but no heat of skin or unusual quickness of the pulse. Under the simple treatment of a diaphoretic mixture, this patient had recovered by the 7th of September. The author who relates this case, Dr. R. HAINES, remarks that cases of this kind are not uncommon in India, the idea of the poisonous nature of pounded glass being even more generally received amongst the natives of India than of Europe. It is generally taken for the purpose of committing suicide. When such large fragments, slender and sharp-pointed, have been swallowed, some degree of danger must always exist of the pieces striking cross-wise in some portion of the intestinal tube, and producing ulceration, which might lead to the laying open of a large vessel, or of the cavity of the peritoncum. Hence it is always advisable to keep the patient under observation for ten or twelve days.—*Transactions of the Medical and Physical Society of Bombay, 1855.*

II. WOUNDS AND INJURIES.

Injuries of the Head.—H. J. CARTER, Esq., coroner's surgeon, Bombay, records three very curious and instructive cases of injuries to the head, and their results.

CASE I. Hemorrhage and Death from Laceration of the Parietal Vein.—A Parsee, in a fit of intoxication, fell with his head against a stone; this caused a wound, from which blood began to flow. He was taken to a liquor shop, and placed on a bench in a small room, about two o'clock in the morning. There he went to sleep, and was not seen again until five a.m., when the servant found him breathing heavily, and at seven he died.

Post-Mortem.—About two pints of blood were found under the head. A small lacerated wound, about an inch long, was found over that part of the sagittal suture where the parietal foramen generally exists. This wound had not extended to the bone, and although insignificant in appearance, it was obvious that the blood had come from it. On raising the scalp, no extravasation of blood appeared on any part of the cranium except opposite the wound, where there was a spot about two inches in diameter. The parietal vein was evidently ruptured, and to give an idea of its size, "the foramen by which it entered the cranium was about a twelfth of an inch in diameter." There was no fracture of the skull. The brain appeared to be more congested than usual. There was no extravasation of fluid in the brain.

The author, in remarking on this case, states that at the inquest he gave it as his opinion that the deceased died from hæmorrhage, but says that, on reflecting upon the subject, he thinks he should modify this opinion by adding, the man might have died from apoplexy brought on by drinking spirituous liquors, and the effusion of blood have taken place after his death, since experience shows that, in India, where the blood becomes rapidly fluid, nearly all the blood in the body will occasionally flow out of a small wound of the head or neck within a few hours after death, if the body be in a horizontal position. This is a very delicate medico-legal point. At the same time, it is not very easy to comprehend how the blood from the lower half of the body could escape from a wound in the upper half, when the heart has ceased to beat, seeing that it could only arrive at the bleeding point after having passed through the right side of the heart into the pulmonary circuit, thence back to the heart, and so through the arteries to the head or neck. Is there motion of the blood after death in India? or can fluid venous blood find its way through the veins in a direction just the opposite of its usual course?

CASE II. Death from Wound of the Vertebral Artery.—A police nâique was stabbed in several places by another policeman. The wounds were inflicted with a double-bladed clasp-knife, both sides of which must have been open, as the wounds were double at each place. He was suddenly heard to cry out, and on another policeman coming to the spot, he was found sitting on a bench, with blood flowing freely from a wound in the neck. He soon became insensible, and died about an hour after the infliction of the wounds.

Post-Mortem.—There were several marks of violence externally. On the right temple and on the left side of the neck, midway between the ear and the acromion, there were in each spot two wounds, one about three quarters, the other about half an inch in length. All were superficial, except the two in the neck. Here the largest and deepest passed behind the carotid artery and internal jugular vein, and between the œsophagus and vertebral column, terminating on the right side of the latter, but injuring nothing of any consequence in its course. The small wound led directly to the interval between the transverse processes of the third and fourth cervical vertebræ, where it ended by dividing the vertebral artery. The body was healthy. When the clasp-knife with which the wounds had been inflicted was examined, both blades were found open, and the little blade bent to one side, which accounted for the difference in the distance between the double wounds at the different places, and the direction which the small blade took to reach the vertebral artery.

CASE III. Fracture of the Odontoid Process, and Dislocation of the Second Cervical Vertebra.—An engineer apprentice on board-ship was playing with a shipmate, when he tripped backwards over the chain cable, and slipped down, after which he looked up and laughed, and said, "Help me up." On trying to rise he drew his arm out of the hole through which the cable passes down into the hold, and suddenly becoming convulsed, gave a few gasps, and died. He did not appear to fall in the first instance with much force. For six months previously he had seemed to be in good health.

Post-Mortem.—Dr. SHEPHERD, who examined the body, found on opening the back part of the neck a rupture of the ligaments connecting the first and second vertebræ with each other and with the head, and also a dislocation of the second vertebra, the odontoid process of which was fractured, and pressed on the spinal cord. The body was healthy. The fall appeared to be attended with so little force that the fracture was attributed by some to the efforts made by the boy to recover himself in the act of falling.—*Transactions of the Medical and Physical Society of Bombay, 1855.*

Death from Rupture of the Bladder following a Kick.—Dr. HENRY PORTER, of Peterborough, relates the case of a man, aged 36, who received, in a fight, a kick in the abdomen. He lost all consciousness until he was carried home, when he

gradually recovered his senses, but remained in great pain all the night and the day following. When seen on the following evening, he had passed no urine. A catheter was introduced, but a flow of urine could only be produced by placing him in the erect position, when about four pints were drawn off, very highly tinged with blood. On the succeeding three days during which he lived no urine was passed, except through the catheter and in the erect position. There was no tenderness of the abdomen nor tympanitis, except on the last day, when some tenderness was present. On the morning of the day on which he died (the fourth after the receipt of the injury), he was seized with bilious vomiting. He died during the night. On post-mortem examination, there was a slight bruise externally, between the umbilicus and the anterior superior spinous process of the left ilium; and blood was extravasated between the abdominal muscles at this spot. The small intestines were distended with gas, and firmly agglutinated. The omentum was drawn up. The bladder had a large aperture in its posterior and upper surface, above the reflection of the peritoneum, of sufficient size to admit two fingers. There was no trace of any previous disease of the organ.

Dr. PORTER remarks that the points of interest in this case are—

1. The absence of signs of peritonitis during life at all commensurate with the amount of inflammatory action observed after death;
2. The length of time which elapsed between the receipt of the injury and death;
3. The question whether the rupture was complete at first, or whether the peritoneal coat gave way at a subsequent period. Dr. Porter believes that the rupture was complete at first, for the following reasons:— First, there was a degree of collapse about the patient from the first; secondly, there was no change in the symptoms from chronic to acute, which would have probably occurred if the peritoneal coat of the bladder had given way subsequently to the injury; thirdly, there was no increased intolerance of pressure nor excess of pain a few hours before death; fourthly, the rupture was very large, and in the situation in which a catheter would pass directly into the cavity of the abdomen; fifthly, it was never possible to obtain urine through the catheter, till the man was placed in the erect position.—*Association Medical Journal*, June 22, 1855.

III. MISCELLANEOUS.

Diagnosis of Blood-Stains on Old Clothes.—Dr. ALBERT, of Euerdorf, relates the following case:

A Jew pedler, named Seh. von O., complained before the authorities, that an individual, by name D. von S., had stopped him in a wood near Euerdorf, and, presenting a pistol, had threatened to shoot him if he did not immediately hand over the money he had about him. Although exceedingly surprised and alarmed, complainant stated that he did not lose his presence of mind; on the contrary, he sprang upon his assailant, snatched the pistol from him, and gave him two heavy blows with it on the back of his head, so that the assailant was glad to run off and hide himself in the wood. Complainant said further, that through these two blows the accused must have received a serious injury on his head, and must still bear on portions of his clothes traces of the blood which flowed from the wounds. The accused having on this information been taken into custody, was brought before Dr. Albert. The following was the result of an examination instituted five days after the commission of the alleged crime:

1. The whole of D.'s head was covered thickly with variously-coloured scabs, but there was no wound to be found on it. If such wound had really existed, it must have taken place under some rubbed-off scabs, which must have been speedily renewed; this was all the more probable, as on the spot where it was alleged that the blows had fallen, there were some scabs of a blood-red colour, and seemingly composed for the most part of dried blood. The other parts of the

man's body were covered, but not so thickly, with the same sort of scurfy eruption.

2. On the back of the man's linen jacket there were found twenty-six red stains, each about as large as a halfpenny, and a few more, which had been wiped away, on the hips of the trousers.

As D. declared that these stains resulted from his occupation as a lime-washer, the following examination was made :

1. Thirteen of the stains had a shining, and twelve a dull appearance.

2. Microscopically, the shining stains presented a fibrous sort of tissue hanging together in a mesh. The dark ones, on the contrary, had a looser and more pulverulent consistence.

3. Some of the shining stains, when rubbed, lost their colour, but remained as a tough yellow-brown mass; while the dark stains fell away altogether into powder.

4. Placed under water, the shining stains altered; the red constituents separated, and sank to the bottom without communicating any tinge to the fluid; but the red part of the dull stains was quickly and completely dissolved in water, imparting a red tint.

5. When the two fluids thus produced were tested with caustic ammonia, the first underwent no alteration; the last experienced a change to a violet-brown colour.

6. Nitric acid added to these fluids removed, in the first specimen, the colour of the red fibres, forming with them a light-red precipitate; in the last it caused a dark-brown thick discoloration.

7. Tincture of galls produced, after agitation in the first fluid, a muddiness and a precipitate; in the last it produced no alteration.

From these examinations the inference was drawn, that the fourteen first-named shining stains were blood-stains; but that the dull stains were produced by splashes of a red-coloured wash (*Englisch roth in Wasser gelöst*).

Upon hearing this explanation, the accused man changed his story, and stated that the blood-stains arose merely from his having carried a kid on his back, which he had killed by cutting its throat. Upon this, Dr. Albert instituted another inquiry, to ascertain whether the stains were or were not those of human blood.

A watery solution of one of the blood-stains was treated after Barruel's process with sulphuric acid; and a solution of another was treated as proposed by Friedberg—i.e., it was evaporated in a glass tube over hot coals. Thus treated, it gave forth an empyreumatic odour, but no other specific smell. The fluid that had been treated with the acid, emitted an odour similar to that which one perceives in a damp cellar in which cheeses have been kept.

A few drops of blood were then taken from the accused man, and were treated in a similar way; for the most part, the same results were obtained. The man's scurfy eruption emitted a similar smell.

The blood of several healthy persons was examined in a similar manner in larger quantities. A smell was only caused by evaporation, and this was intolerable.

Goat's and buck's blood, from old and young animals, were subjected to the same test; but nothing peculiar resulted.

Upon these experiments—which, by the way, are most unsatisfactory and loose—the conclusion was arrived at, that the blood-stains were probably not derived from a goat, but rather from the man himself; inasmuch as, on the treatment of his fresh blood with sulphuric acid, the smell emitted was so strikingly like that of his own perspiration and urine.

After the inquiry, the confession of the man proved that the blood-stains did arise from wounds in his own head, caused as the accuser had described.

From further experiments by Dr. Albert, he infers that the statements of Barruel and Friedberg, that the blood of different animals, treated as proposed by

them, would betray itself by a smell peculiar to each, is not worthy of credit, and is inapplicable to judicial inquiries.—*Henke's Zeitschrift*, 1855, p. 392.

Infanticide in Prussia; its Detection and its Punishment by Modern Law.—Dr. VON HASELBERG, of Stralsund, concludes the narration of eight cases of infanticide with some medico-legal remarks.

In late years there have been several cases of infanticide in the district of Stralsund. In the eight cases related, pregnancy was denied, parturition was concealed, and the child destroyed. In one instance the mother died; in two cases, the legal proceedings were stayed; in three, the murderesses were declared not guilty by the jury; in one case, a verdict equivalent to "not guilty" was returned; and in one case only was the culprit found guilty. Paradoxical as it may appear, it is nevertheless true, that infanticide is permitted by the Prussian laws. By a clever use of the law as there administered, each unmarried pregnant woman may kill her child and remove it without becoming liable to punishment. She can deny her pregnancy; and knowing this, she resists all inquiry and examination, to make which no one has a right. She must then quietly wait till her confinement. After birth, the mother, without using violence, has only to cover the child carefully with the bed-clothes, and thus induce death by asphyxia. The first breath can with difficulty be prevented. The hydrostatic test affords evidence if the child has breathed and lived. By good fortune, the medical examination may prove only that the child has *probably* lived, and may have died of apoplexy induced by pressure on the head during birth. There are no marks of injury. The fact of murder cannot be proved, and the proceedings are stayed. Or the case may come before a jury (*Geschworenen*), and as soon as the jury hear anything of "probable" or "very likely," or perceive the least hesitation on the part of the medical witnesses, they assuredly return a verdict of not guilty.

Dr. Haselberg further observes, in a general way, that jurors, in his part of the world, are not particularly acute as to the value of evidence, or the skill of medical witnesses. It is usual to summon a second or a third skilled witness; but this may have only the effect of rendering the evidence more doubtful. If there be a difference of opinion, the evidence of the witness who is most popular or eloquent has most weight, though it be of the least real value.

In all these respects, the results obtained by the Prussian medico-legal tribunals are not unlike those obtained by the same tribunals in our own country.—*Henke's Zeitschrift*, 1855, p. 310.

The Composition and Physiological Action of the Water recently used in the Durham County Jail.—A paper on this subject appears from the pen of J. F. W. JOHNSTON, Esq., F.R.S. It seems that the well from which the water was taken has a depth of eighty-four feet, and is situated among the shoals and sandstone of the coal measures on which the city of Durham stands. It is built inside with stone and cement, and the water at present stands in it to the depth of twenty-one feet. Over the water rests an atmosphere of carbonic acid gas.

The pipes formerly used in connexion with the pump of this well were made of lead, but after a time these were eaten into large holes in various places, and coated inside with an incrustation, which is said to have been nearly an inch thick. In 1851 these pipes were removed, and replaced by pipes of cast iron.

The water when recently drawn has no sensible smell, and only a slightly saline taste. When heated in an open vessel, it speedily became covered with a thin white film; when boiled, it assumed a milky appearance, and gave a white deposit of sulphate and carbonate of lime.

When the well was sunk, no analysis of the water was made; and for ten years it was, with a few short exceptional periods, constantly used for the food and drink of the prisoners. By and by, Mr. Shaw, the medical officer of the prison, began to observe that certain peculiar and striking symptoms were manifested amongst the prisoners. Men in all classes suffered equally. These symptoms

were those of glandular enlargements of the neck. Mr. Shaw thought that the water of the well was the cause of these peculiarities, but the fact did not come out accurately until six months later, when the pump being out of order, and filtered water being supplied from the river Wear, a marked improvement at once took place in the health of the prisoners. An analysis of the pump-water was therefore instituted before its use was recommenced. The results ran thus:— An imperial gallon of the water, evaporated to dryness, left of solid matter, dried at 270° Fahr., ninety-four and a half grains. When dried at only 112°, the solid matter amounted to upwards of a hundred grains. The solid matter consisted of—

	Grains.
Sulphate of lime (anhydrous)	31·38"
Carbonate of lime	15·35
Sulphate of magnesia	4·49
Carbonate of magnesia	1·48
Chloride of magnesium	74·01
Chloride of sodium	6·19
Silica	1·28
Water retained by the salts of magnesia after drying at 270° Fahr.	16·82
Nitric acid, ammonia, and oxide of iron, of each a trace.	
Organic matter and loss	3·50
	94·50

Mr. Johnston remarks on this analysis, that the quantity of solid matter which the water contained alone rendered it, according to our present views, entirely unfit for domestic use. At present, waters are considered already somewhat impure if they contain from twenty to thirty grains of solid matter to the gallon. He also observes, that the chemical nature of the solid contents of the water had quite as much to do with its observed effects as the quantity it contained. He refers especially to the amount of sulphate of lime. He refers to the fact that the prevalence of goitre in the Derbyshire and Swiss valleys has been ascribed to the large quantities of magnesian limestone said to be contained in the ordinary drinking and culinary water used in those parts, derived as it is from mountain limestone districts. Mr. Johnston very properly connects these facts and inferences with the peculiar case which he has described, and intimates that the physiological effects produced by these waters may be connected with, or arise as a consequence of, the united action of the gypsum and the chloride of magnesium. In consequence of Mr. Johnston's report, the use of the water is discontinued in the Durham jail.—*Edinburgh Monthly Journal*, May, 1855.

In both a physiological and a pathological sense, the report given above is of deep interest; but it is quite in vain to attempt to unravel the problem involved from the data given. In the first place, it must be ascertained whether the chemical ingredients stated, really exist in the same condition in the water as it was drunk, and as they were found in the crucible. Secondly, particulars are wanting as to the manner in which the water was applied. Was the water drunk by the prisoners, as a general rule, freshly drawn from the well, or after having been boiled? And thirdly, might not some other agent have been driven over with the distilled water, of which we have as yet no account? As the water of the well still remains for observation, and its peculiar physiological effects are thoroughly brought out by a grand accidental experiment, it is to be hoped that the opportunity will not be lost of instituting a series of philosophical inquiries in relation to effects so obvious and suggestive.

HALF-YEARLY REPORT OF MICROLOGY.

By JOHN W. OGLE, M.B. Oxon., F.R.C.P.

Curator of St. George's Hospital Museum, and Physician to St. George and St. James's Dispensary.

PHYSIOLOGY AND ANATOMY.

I. HISTO-GENESIS OF PARTS.

Epithelium.—GUNSBERG* (quoted from Canstatt's Jahresbericht) finds that the epidermal cells are completed in the fifth week of foetal life; and from the fifth to the tenth week, in the most superficial layers of epidermis he found textureless membrane, with cell-formation interspersed, elementary corpuscles and molecules, from which he concludes that death of the most superficial epidermal layers occurs in this period. Cylindrical epithelium was seen by Günsburg to be developed about the eighth week of foetal life in the respiratory and digestive mucous membranes; and he observed a layer of epithelial nuclei on the anterior surface of the capsule of the crystalline lens in a foetus of four months old.

Nails.—GUNSBERG states that, in the embryo of thirteen weeks, the nails consist of lengthened nucleus-holding cells. The *nuclei* are dissolved by acetic acid, the cell-membrane remaining.

Pigment is said by GUNSBERG to be developed as a finely granular covering of the cell-nuclei. The aggregation of the pigment of the iris is said by him to be enclosed in cell-walls at the termination of the second month. There is a case in the Boston Medical Journal, April, 1854, in which the dark colour of the negro's skin is said to have been absorbed.

Lenses, &c.—GUNSBERG† states that, in a foetus six weeks old, he found the entire lens to consist of large transparent nucleus-holding cells. In the seventh week, there were fibres formed in the central parts; and at the eighth week, the fibres were partly entire and partly split.

Connective or Areolar Tissue.—According to HESSLING and REICHERT, the development of this tissue proceeds in all cases from an intercellular substance. Reichert‡ entirely refutes the statement of Kölliker, that there is a transformation of the spindle or stellate cells of the embryonic areolar tissue into a net-like, or simply cylindrical bundle. He does not deny that, in various forms of embryonic tendons, isolated cells are to be discovered. The chief mass at first is entirely tenacious, finely granular, afterwards obscurely striped, and only artificially divisible into fibrils or fibres, in which elongated oval nuclei are distributed. The spindle-shaped cell-membranes, which are indistinct, and grow into the so-called nucleus fibres, belong to the nuclei.

LUSCHKA§ considers that connective tissue fibres arise out of the direct conversion of a homogeneous substance, for whose universal validity he has contended, at least as regards the fibro-cartilaginous layers of the pelvic joints; but this observation does not apply to the developmental processes. It was easily seen how the homogeneous fundamental substance of the hyaloid cartilage became changed into fibrils of connective tissue, between which the cartilaginous bodies were placed. Günsburg says, on the contrary, that in a fifth-week embryo, the dermal fibres are developed out of united fibre cells, and many such fibres become so closely united to each other, that at a later period they appeared as anastomosing fibres. The dermal fibres differed from other connective tissue by much thinner and more delicate fibre-plates.

Fat.—According to ROBIN,|| the covering of fat cells is developed last of all the parts; and in atrophy of fat cells it is the first to depart.

* Verhandl. der Kaiserl. Akademie, Band xvi. Ab. 1, p. 264.

† Untersuchung über die Entwicklung verschied. Gewebe des Körpers.

‡ Canstatt's Jahresbericht, Band i. p. 27. 1854.

§ Arch. für Anat. et Phys., Band iii. Heft 2, p. 302. || Gazette Médicale, No. 16, p. 24.

Blood.—DRUMMOND* seems to deny the position, that the nucleus alone of the original embryonic cell was transformed into the perfect blood corpuscle, such as has been advocated by Wharton Jones and Vogt.

In the blood corpuscles of the larva of the frog, formed out of embryonal cells, as well as in the lymph corpuscles, Drummond noticed that the fat granules which they contained diminished in proportion to the appearance of colouring matter within them; and from this fact, as well as from the observations of Weber on the destiny of the yolk-remains of the chick, he concludes that the colouring matter of blood is formed out of fat.

Drummond found slight fibrin coagulum in the heart of the embryo chick, about the third or fifth day.

Bloodvessels.—The formation of capillaries from star-shaped cells, as advocated by Schwann, is agreed in by Drummond, Günzburg, Kölliker, and Bruch (vide Caustatt). The two latter recommend for observation the tail of the tadpole, as also the fetal crystalline lens capsule and the allantois. These authors, however, disagree in some points. Kölliker says that the primary origin of bloodvessels is by tubercles from the apposition of rounded angular cells in a straight line, and the absorption of their septa; and that from these tubercles proceed delicately-pointed processes, which, elongating, meet similar processes from stellate cells in the surrounding tissue, and unite with them. At the same time, the other processes of these cells unite, so that a network of the star-shaped cells is formed, continuous with the already-formed capillary trunks. Bruch denies the formation of tubes out of the rounded-shaped cells in the first instance. He also finds that the number of nuclei in the walls of the capillaries does not answer to the number of formative cells, but thinks that an increase of nuclei occurs, by division, after the union of the cells. Bruch also found that, as a rule, the projections from the star-shaped cells which are united into capillaries are arranged, not in a radial manner, but so that the spindle shape of the cells remains ascertainable. He investigated the peripheric extensions of bloodvessels in very small swine and ruminating animals, and saw how the spindle-shaped cells which form the vascular parietes, towards the finest ramifications always become more scanty, until at last but a few, sometimes only a single one, exist. These, which were stronger in proportion to their number, often had thread-like processes and anastomoses so fine, that one would not have imagined them to be hollow had not blood corpuscles been seen within them in places. In the chorion papillæ of the swine, Bruch observed that the formation of the vascular loops of which they are composed differs from that in rabbits and men; that fully-formed bloodvessels existed, and papillæ pushed out from the chorion before the bulgings took place, so that he describes this species of formation of the finer, not properly capillary vessels, as a kind of budding analogous to the production of glandular vesicles. The choroid plexus seemed to have a different formation.

Bruch found the cerebral capillaries fully formed in the bullock's embryo, one and a half inch in length. He states, that the spindle-shaped cells which at first compose the walls of the non-capillary vessels correspond to the inner vascular coat only; to which are added subsequently the annular fibrous and adventitious coat, formed out of new cells; whilst from within, as a secondary production, an epithelium is formed which, even in a very small embryo, can be recognised. This epithelium is said to exist, by Ségond and Robin, in the embryonic fine arteries and veins, but after birth it ceases to exist there. In mammalian embryos, Drummond found the spindle-shaped cells composing the walls of the larger vessels to be arranged in a direction exactly opposite to that described by Bruch. Most of them were placed more or less transversely to the longitudinal axis of the vessels, and only a few existed at right angles. In an embryo four inches long, distinct fibres, formed by the union of spindle-shaped cells, existed in the outer part of the vascular walls. Epithelium was perceived in the interior of the vessels, and this

* Monthly Journal, Sept., p. 214, and Nov., p. 355, 1854.

layer, in embryos eighteen inches long, still possessed its primitive appearance. In these embryos, in addition to an annular fibrous coat, Drummond says that a longitudinal fibrous coat existed (which, in Bruch's opinion, was the first one laid down), but not yet as a connected membrane. Kölliker thinks the development of the larger vessels, by the metamorphosis of capillaries, to be very frequent; and for all greater vessels arising after the completion of the organ, he lays claim to this method of formation entirely.

Muscle.—The abdominal muscles of a four-weeks' embryo appeared to consist of clear smooth tubes, according to GUENSBURG (op. cit.), of a greyish tint, capable of being split by pressure into the finest fibrils, and containing largish oval nuclei. The cross-stripping was clearly to be seen. In a seven-weeks' embryo, the small muscles of the neck, the scaleni, intercostals, and psoas muscles, consisted of perfect striped primitive tubes; and in the muscles of the extremities the primitive fasciculi were formed out of united cells: yet, he thinks that the nuclei of these cells form the fibrils, and the fibril-plates originate from a melting down of the nuclear molecules in a direction from the centre. In a ten-weeks' fetus, the formation of the sarcolemma was decidedly distinguishable from the formation of the primitive muscular fibre. In an eight-weeks' embryo, the muscles of the back were the most developed; and in a three-months' one the glutæi were. The ten-weeks' fetus possessed perfect primitive fasciculi in the diaphragm and tongue, and those of the heart were half as thin as those of the glutæi, their development being from fibre cells; yet they had no cross-stripping, which was found in the heart of a three-months' embryo. Gunsburg recognised in the cleft membrane of the fibre cells, already united with fibres, the planning of the cleavage-splitting of the primitive bundles. He thinks the primitive fasciculus of the heart's muscle is formed out of a single fibre cell, whilst all the other muscles arise out of fibre cells from three to eight in number. He says that the smooth intestinal muscular fibres are formed out of a single cell, between the ninth and thirteenth weeks.

In a paper by SAVORY, in the 'Transactions of the Royal Society,' Jan. 11, 1855, the development of muscular fibre immediately beneath the skin of the fetus of the pig, other animals, and man, is described. The nuclei, round or oval, became arranged and adherent, and then became invested by blastema, so forming elongated masses, the nuclei observing no regularity. The nuclei subsequently fell into rows, and became clearer, at this stage resembling the permanent form of many insects. The fibres elongate, the nuclei separate, and the fibres decrease by the falling in of the lateral bands, formed of condensed blastema. The nuclei fade away in many cases, and the fibre increases by accession of surrounding cytotoblasts, which attach themselves externally, and become invested by blastema.

Nervous System.—The development of the axis cylinder in nerves, according to HARTING,* does not correspond to the original contents only of the embryonal cell, but also to the cell membrane; and the nerve-narrow deposits itself between the first secreted elastic sheath and the proper cell membrane, so that the last may be compared to the primordial membrane of plant cells.

Cartilage (Development of).—According to GUENSBURG the parietal membrane is developed in the form of spheroidal vesicles in the germinal layer of the formative cells. In these vesicles no discreet nuclear mass is distinguishable, nor is any molecular matter visible in their interior. The earliest non-nucleated vesicles remain in the middle of a layer of formative cells, unless they pass into a blastema, providing for a secondary development. The residuum of blastema becomes an intercellular mass, as is seen in ossifying cartilage, where a clear plaited membrane is formed. The form of cartilage cells is determined either indirectly by the displacement of intercellular substance, or directly by means of the fibrous capsule membrane.

* See Canstatt, Band i. p. 63, 1854.

Glands: Salivary Glands.—The sub-maxillary gland of a four-months' embryo was found by ECKER to contain pear-shaped acini, which, gradually diminishing, pass into the twigs of the excretory ducts; and out of these acini in course of development the indented blind terminal canals of the perfect organ appear to proceed.

Seminal Glands.—The formation of the seminal canals, according to BRUCH,* begins in the bullock somewhat earlier than those of the renal canals, in the shape of very short, proportionately wide, round or oval, groups of formative cells, which at first may easily be pressed from each other, then become more firmly connected together as the formation of sheaths goes on, and grow in length with them, but in the end continuously pass into the indifferent formative tissue. Along with the extension of this membrane, an inner cavity is formed, whilst the cells extend, epithelium-like, on the wall. By growth in length arise numerous windings, of which at first nothing is visible.

Kidneys: Wolfian Bodies.—In a bullock's foetus, 8" in length, the urinary canals were of variable size, ray-like, and provided with varicose bulgings, according to BRUCH. In the Wolfian bodies, Bruch found the tunica propria to arise from united cells, and the gland vesicles appeared to develop secondarily, by the growth and bulging out of the membrana propria.

Liver.—In the embryo of the chick, REMAK† observed round colourless transparent bodies, with a smooth membrane surrounding them, a thick wall consisting of delicate concentric layers, and containing a sharply-defined cavity filled with nuclei. This cavity was about one-third of the diameter of the entire cell, and contained nuclei varying in number up to thirty-two, furnished with simple or double nucleoli. The nuclei varied in size in reverse proportion to the age of the embryo, withstood acetic acid, the enclosing membrane swelling out, the parietal substance becoming transparent, and losing its laminated character under the use of water, acetic acid, and alkalis. The cells shrank under the use of chromic acid, alcohol, and solution of corrosive sublimate, and assumed a brownish colour under iodine solution, but did not become blue under sulphuric acid. In larger embryos, cells having two cavities containing nuclei were found. About fifty embryos above the size of one inch were examined, and all parts of the liver exhibited the same appearances. Before dissolution, the many-nucleated cells divided into cells with single nuclei. Then appeared cells with groups of nuclei more separated from each other, also smaller cells with less nuclei; and finally, cells with two or one nucleus, only distinguishable by their having a larger circumference, from lymph cells. These appearances were observed up to the twelfth day in newly-born rabbits, when, owing to the softened condition and tendency to destruction, the investigation becomes difficult. In the chick, this change begins as soon as absorption of the yolk begins. The above-mentioned cells are considered by Remak to have nothing to do with the hepatic cells, but to be rather elements of the connective vessel and nerve-holding fibre layer of the liver. They seem to have no connexion with the bloodvessels, and are in vain sought for in the blood at the time of their dissolution. Their function is (in the author's mind) most probably to fill up the cavities of the liver before the liver-cylinders extend themselves into lobules.

Spleen.—This viscus, up to the twelfth week, is composed, according to GUNSBURG, of indifferent formative cells. In the thirteenth week, globular vesicles were seen, with from two to three nucleoli, just like the globules, he says, of the adult spleen pulp, blood corpuscles also, and manifold transition of formative cells into fibres. Capillary, as well as larger, bloodvessels were laid down. At the sixth month the transitional form did not exist; the trabecular scaffolding consisted of numerous ramifying bundles of nucleated fibres of connective tissue, and there were large blood corpuscle-holding cells. Günsburg considers the existence of spleen fibres before the last week of foetal life to be problematical.

* Canstatt's Jahresbericht, Band i. p. 70. 1854.

† Müller's Arch. für Anat. Phys. und Wissensch. med., Heft 1, p. 99. 1854.

II. HISTOLOGY.

Epithelium.—The epithelial layer of the *endocardium*, which LUSCHKA and TODD and BOWMAN speak of as composed of two layers, is thought by KÖLLIKER to be only composed of a single layer—the second or deeper one being, in his opinion, owing to the commencement of a diseased thickening of the endocardium.*

The anterior surface of the *iris* is asserted by Kölliker to have a layer of epithelial cells upon it (just as the posterior surface possesses one, the uvea, of pigment-holding cells), which in the newly-born, or in young subjects, are rounded, large, and flattened, constituting, when seen in a fold of the iris, distinct slight elevations. This assertion is opposed to the observations of Mr. Bowman, who thinks that there is *no* epithelium on the anterior surface of the iris. Kölliker even gives to this supposed epithelial layer a partial share in the production of the varied colours of some irides. An epithelium is stated to exist on the anterior surface of the capsule of the crystalline lens by Lohmeyer and Brücke, but this is denied by Kölliker, who asserts that there is one only on the deeper surface, composed of polygonal cells, which after death burst, and furnish the aqua Morgagni.

A distinct epithelium was obtained by LUSCHKA, of Tübingen,† out of the pubic and iliac bone-articulation, which the author looks upon as a true articulation. It consisted of rounded cells, finely granulated, and generally containing a clear, refracting, and delicately-defined nucleus. Often the nucleus was invisible, and the corpuscle appeared quite granulated and irregular, possessing much finely granular molecular matter. Acetic acid rendered these cells almost invisible, and they were destroyed by strong potash solution.

Luschka also perceived epithelium of the venous textures, with thorn-like processes projecting sometimes free above the level of the other epithelial cells. These he considers to be agents in the transudations of serous fluid, thinking it improbable that in any normal process nature should act in this matter by simple mechanical transudation, seeing that as no impediment would exist to further transudation, the amount of such fluid-formation would be unlimited. As to this supposition, Henle opposes Luschka. Moreover, Luschka brings forward the ascertained chemical properties of the secretions of serous membranes, as showing that something more than a simple exudation of the blood serum occurs. Luschka supposes that these cells have the secretion formed in them, and that they are then separated. He explains the process by microscopical observations of the venous tissues of newly-dead mammalia and birds. He says that the free surface of the venous tissue contains different epithelial elements, which are perceived to be only a succession of one and the same fundamental pattern, the finely-granulated nucleus holding pavement epithelium. Besides these, there are, in all possible transitions, scantily-granulated spherical bodies, with a nucleus. 2dly. Spherical and, excepting a single contained nucleus, transparent homogeneous and very delicate cells. And 3rdly. Vitreous-looking vesicles, without corpuscular elements, with very delicate structureless walls, and mostly of very large size. These latter bodies are so pellucid and delicately-defined, that they can only be recognised by a suitable illumination. The final behaviour of these pellucid bodies is of the greatest importance. They are seen oftentimes to extend themselves, and at last to burst or dissolve; and out of them smaller and larger transparent drops, like fluid albumen, are seen to issue, which gradually become mixed with the water added to the object. It may further be seen how the cells in this way leave behind them no débris, but are totally dissolved, or only leave a very fine membranous coagulum. In the above description we recognise, according to Henle, the hyaline or albuminous drops which were first seen by Dujardin to proceed out of the walls of infusorial bodies, and which have been since observed in many other higher and lower animals. They were figured by Donders among the epithelium of intestinal villi, under the name of mucous globules.

* See Canstatt, Bd. i. p. 27. 1854. † Arch. für Phys. Heilk. See Canstatt, Bd. i. p. 27. 1854.

MAYER* asserts that, in his opinion, small canals are formed amidst the fine granules which fill the epithelial cells, through which absorbed fluid is drawn by means of some contractile power of the nucleus, which thus becomes the chief organ of imbibition. We need hardly say that the idea is entirely hypothetical.

Here we may allude to a paper by VIRCHOW, in his 'Archiv,' April, 1855, On Cell Pathology, in which he dwells on the irritability inhabiting certain cell-elements, and quite independent of nervous influence; and quotes the observations of Leydig, who shows that the contractile substance of the lower animals, the so-called sarcode, is contained, as in the arm-polyps, in cells.† Virchow indeed claims irritability for all cells and cell nuclei, whether isolated, or grown together so as to form definite structures.

The depressions and markings which are visible on the superficial scales of epithelium, are looked upon by KEBER‡ as pores and chinks, and are considered to be a manifold system of hollow spaces and cavities; and by rubbing in quicksilver, he states that he has witnessed its presence in the pores.

DONDERS§ found that the fatty particles seen in the cylindrical epithelium of intestine during digestion, existed in all parts of the cell. After death they accumulated into large drops. The nuclei were free from fat.

Ciliated Epithelium was seen by VIRCHOW over the surface of all the cerebral ventricles, but it was the most marked in the fourth ventricle. Günzburg states that absolute alcohol quickly dissolves cylindrical and ciliated epithelium, which swell out and become pale in ether. The cilia remain unaltered in hydrated alkali, the cell walls swelling out, and the cell contents being dissolved. We have ourselves lately seen ciliated epithelium lining the frontal sinuses of a man who had been dead fifteen hours, the cilia working away with some rapidity.

Nails.—According to VIRCHOW,|| the lowest cells under the nail were cylindrical, and on the addition of acetic acid extended themselves into lengthened spindle-formed structures, more or less perpendicular to the surface. In the matrix, on the contrary, they were small, flatter, and of a dim yellow colour, with granular contents. The structures, interpreted by Rainey as follicles of the nail stroma, exist, according to Virchow, in a threefold form:—1. As small cylindrical bodies; 2. As large flask or pear-like bodies; and 3. As round concentric masses. All these lie at the bottom of the furrows, between the ridges. The first two were seen by Virchow to be closely connected with the surface. The concentric ones appeared to be entirely independent, and all three consisted of epidermal elements, a special membrane not being recognised. The smallest cylindrical masses contained small cells, disposed in a circular form, with transversely-placed nuclei. In the pear-shaped bodies, the cells of the upper part were arranged transversely; but in the final protuberance, the cells were more parallel with the surface, and contained fat-like glistening masses in the interior. The concentric structures consisted entirely of laminated epidermal plates, or contained a fat-like, glistening, structureless mass. Virchow supposes them to depend on a disturbed development of the retina.

Hair.—It is stated by Mr. P. BROWNE, in the 'Philadelphia Monthly Journal,' March, 1854, that the hair of different human races differs in form. It is cylindrical in the Red Indians, elliptical and oblong in the negro; and in the union of the negro and American, oval and flat intermixed.

Cornea, Lenses of the Eye, &c.—The presence of a true liquor Morgagni during life, which is denied by Kölliker and others, is asserted by LOHMEYER.¶ Kölliker attributes the appearance of a layer of polygonal cells at the back part of the crystalline lens under the capsule to club-shaped enlarged and flattened ends of the lens'

* Verhand. der Rhein. Natur. Verein, Band x. p. 372. See Canstatt, Band i. p. 22. 1854.

† See Müller's Archiv.

‡ Mikroskop Untersuch. über Porosität der Körper. Quoted from Philosophical Journal, 1854.

§ See Canstatt, Band i. p. 29. 1854.

|| Würzb. Verhandl., Band v. Heft 1: quoted from Canstatt, Band i. p. 30. 1854.

¶ Zeitschrift für rat. Med., Band v. Heft. 1, p. 56.

fibres, which are inserted into the inner surface of the capsule, and often leave delicate impressions behind them; and in the same way, the abrupt terminations of the fibres of the lens exist behind the equator of the lens*. K lliker also asserts that the fibres of the lens are, in fact, tubes, which Reichert† thinks cannot be the case, from the completeness with which, on drying, their peculiar polyhedral character is retained. He imagines the fine and longitudinally-striped character which they have to be owing to a folding of their membrane, probably. In the neighbourhood of the equator, the nuclei of the original cells, of the tubes, having grown into vesicles with nucleoli, remain in the centre of each tube. K lliker says that, in the central stelliform figures of the lens its substance does not consist of tubes, but is partly granular and partly homogeneous; and the surrounding tubes gradually lose their distinctness on approaching the central, "stars." The vitreous body is described by DONCAN‡ as consisting of four kinds of elements:—1. A periphery layer of cells, strongly refracting light; 2. Very fine fibres, covered with granules, which were situated more at the sides than in the axis of vision; 3. Numbers of granules, of various sizes; 4. Folded membranous shreds in the anterior part of the vitreous body. The cells, which are identical with those described by Luschka, and considered by Br cke to be undeveloped capillary network in the fœtus, fill the entire vitreous body, and disappear from the centre. Those existing at the periphery are looked upon by Doncan as the result of a colloid or mucin-like metamorphosis of the contents, especially the nucleus; and he thinks that the vitreous humour is developed from these cells, which, partly by bursting and partly by the act of transudation, furnish the fluid. Doncan also thinks the textural parts, the fibre's granules, membranous shreds, &c., to be the result of secondary formations in the mucus formed by such cells. According to HANNOVER (Canstatt), the vitreous body in man consists of sections like an orange, formed by projections of the hyaloid membrane, converging to the centre. Doncan, however, in examining recent specimens without reagents, could see nothing of the membrane in question. He also used specimens hardened by acetate of lead and chromic acid, and in the human body found that the radiated striæ seen on section were owing to membranous septa, but were either indications of zones, as he thought probable from the arrangements of the embryonic cells, or were artificially produced. K lliker thinks that the adult vitreous body consists merely of a mucus-like substance, all trace of the embryonic areolar tissue which it originally possessed being quite lost.

Elastic Tissue.—This is said by HARTING (quoted from Canstatt) to assume a yellow colour on the addition of nitric acid, owing, as he thinks, to the moisture contained. This tissue is considered by K lliker—or at least one intermediate between it and connective tissue—to form the proper choroid of the eye, as it does the ligamentum pectinatum of the iris.

Blood—Solubility of Corpuscles.—DUSCH tried to ascertain§ whether the bile dissolved the blood by reason of its water contained, as Virchow and Henle thought, or by its organic elements. He mixed equal parts of blood and ox-gall together, and in about two hours it became almost clear and transparent. He then found that almost all the blood-globules were dissolved, a few which remained being swelled out and cup-shaped. Again, instead of gall, he used distilled water, and the blood was even more entirely dissolved by degrees. In a mixture of three parts gall and twenty of ox-blood, the blood-corpuscles were all found to be globular, whilst in a like experiment with water instead of gall, most of them retained their accustomed character. The experiment with the single elements of bile proved that the glycocholate as well as the taurocholate of soda, the last especially, possessed a strong dissolving power, whilst after a shorter time the influence of those salts on human and frog's blood was to cause a solution of all

* Zeitsch. f r Wissensch. Zool., Band vi. Heft 1. † Canstatt's Jahresbericht, p. 34. 1854.

‡ Nederl. Lancet, Nos. 11, 12: see Canstatt.

§ Untersuch. als. Beitrag. zur Pathog. des Icterus.

the blood-globules. The nuclei of frog's blood resisted the longest. The taurocholate of soda had the same influence on pus corpuscles. Blood corpuscles were found to be affected by an alcoholic extract of ox-gall in the same way as by those salts; and in order to prove that in this it is probably only the cholic acid, united with taurin, and glycin, which possesses the dissolving power, Dusch established experiments with taurin, by which the blood-corpuscles were seen not to become dissolved, but, after two hours' influence, were only indented.

Enumeration of Corpuscles, &c.—WELCKER, continuing his experiments (some of which were alluded to in our April number, 1854) by means of determinate dilution of the blood,* found that in the three first vertebrate classes the proportion of blood to the weight of body was as one to twelve, whereas Valentin estimates it as one to five. He determines the relative proportion of colourless to coloured corpuscles in the blood to be as follows:

In a healthy girl, aged 19, blood taken from a vein gave the proportion as	1 to 157
Blood taken from a hysterical girl in the same way, gave the proportion as	1 to 506
In blood from Welcker himself the numbers were	1 to 341

In these three experiments, a cubic millimetre of blood contained respectively about 12,133, 8201, and 13,369 colourless corpuscles.

The number of colourless corpuscles in blood has also been carefully and elaborately estimated by MOLESCHOTT.† He finds that, as age increases, the colourless globules diminish in proportion to the red ones. They are increased by a slightly albuminous food in proportion to the like amount of red ones, much more than by food less albuminous; and their average number in man he considers to be about 1 to 35 of the coloured ones. Their number is below the average in those fasting or not menstruating, and in the aged; and in the pregnant, or those menstruating, or in children and young men eating plentifully of albuminous food, the white corpuscles are above the average. Colourless corpuscles were found in the splenic blood of a person one hour and a half after death, in the proportion of 1 to 4.9; but probably cells from the spleen-pulp may have been inadvertently included, owing to pressure of the organ requisite in the examination.

Bloodvessels—Arteries.—The helicine arteries in the penis are considered by SEGOND‡ to be produced artificially by the tearing of elastic fibres which roll themselves up. Such was the view of Henle and Valentin. GERLACH and KÖLLIKER both agree in thinking them true vascular formations, and not produced artificially. They suppose them not to be caecal at their extremities, except in rare instances, and point out offshoots of fine vessels from them which, being further continued, end in venous spaces, intercommunicating.§

Capillaries.—As to varieties of capillaries, SEGOND agrees with ROBIN that there are three different kinds—namely, those which consist respectively of one, two, or three coats, which imperceptibly pass one into the other, and under which are included the finest arteries and veins. In the second variety are transversely-placed nuclei, but not in any of them could Ségond perceive any inner epithelium. Robin says that the transversely-placed nuclei of the second class are more scanty in the capillaries of the brain, spinal cord, and medullary tissue of bone. He denies, along with Verneuil (*le système veineux*), the elastic longitudinally-stripped or fenestrated coat of veins.||

Lymphatic Vessels.—According to GERLACH, the membrane of the central chyle vessel of the intestinal villi is entirely structureless. These villi, according to Brücke, in many animals, as weasel and mole, contained vessels branching out like

* Prager Vierteljahrschr., Band iv. p. 11.

† Wiener Wochenschr., No. 8: from Canstatt.

‡ *Traité d'Anatomie Générale.*

§ See Canstatt, Band i. p. 46. 1854.

|| *Ibid.*

network; these vessels were light in colour by reflected light, and dark on transmitted light, differing in arrangement from the transparent chyle vessels, and looking like bloodvessels filled with small brown granules. Contents of the vessels of the villi, similar to these, were observed by Virchow in the human body, consisting in one instance of large-sized angular granules. These granules were not chyle or fat, as they were soluble in alcohol, and insoluble in ether. Some time ago, Brücke and Gerlach had stated that the blood capillaries of the villi were filled with fat-granules, and thus had the appearance of branched lymph vessels. Possibly both observers had examined preparations like those described more recently by Virchow and Brücke. Kölliker withdraws what, in 1852, he stated respecting the origin of lymphatic vessels in the trachea; he thinks it possible that such vessels were only bloodvessels filled with granular matter. He states that the deeply and longitudinally-situated muscular fasciculi of the adventitious coat of the lymph vessels, which are found on vessels of not more than 1-10^m, are distinctive between them and small veins. DONDERS recognises the muscular elements in the covering of the lymphatic glands described by Heyfelder, but not witnessed by Kölliker.*

Muscle.—The primitive bundles of striped muscle, according to HARTING,† consist of ellipsoid bodies, arranged in a linear direction of from 0·0001 to 0·0017 millimetre in thickness, and from 0·0002 to 0·0035 millimetre long, filled with fluid, as he concludes, from the alteration in form assumed by them during contraction of the fibrils. By means of a stimulus, they became cylindrical instead of ellipsoid. They are imbedded in an intervening substance. In muscular fibres which have lain for some time in hydrochloric acid, the elements separate in a longitudinal direction, often so complete that the juxtaposition of the elements which lie in a transverse plane alone remains, and constitutes the well-known disc. Muscular fasciculi, which are beginning to be affected by the gastric juice, show the same changes. DONDERS states that the sarcolemma of the heart's muscle is rendered very clear by treating it with diluted hydrochloric acid; and according to Harting, the fibres of the heart's muscle, when seen on transverse section, are found to exist in radii proceeding from the central point of the fibres. This same arrangement has been seen in other muscles by KÖLLIKER. Some remarkable structures, discovered by Purkinje' under the endocardium of ruminants, and adopted by Kölliker under the name of "cross-striped muscle cells," have been described by HESSLING.‡ He terms them "grains," and met with them in greatest abundance in the left ventricle, under the endocardium; also in the substance of the heart's walls, especially in the chamois; and not seldom in the outer part of the heart, under the pericardium. These grains, so-called, lying thickly near each other, compose a gelatinous kind of network, visible to the naked eye, under the endocardium. The threads composing the network are surrounded by a sheath, which is—in the sheep, for instance—formed out of a thin structureless membrane, furnished with nuclei; but in other instances consists of a highly vascular connective and elastic tissue, connected with that of the endocardium. The "grains" are described by Hessling as being generally of a rhombic shape, very transparent, and of a waxy consistence. They are sharp at their edges, finely granular, and generally striped in various directions—occasionally being filled with fat granules. In their centre lie from one to three roundish vesicular or granular nuclei with nucleoli, mostly surrounded by fine grey or golden-coloured glistening pigment granules, which in the chamois are very numerous. The nuclei increase by division, and the striped appearance seems rather to answer to foldings of the surface, or shallow impressions. By the use of acetic acid they swell out, the cross stripes being rendered more clear—as they are also by nitric or hydrochloric acids and alcohol. True capillaries, but no nerves, were found amongst the above-named network. There seems to be much doubt as to the character of these grains, which are looked upon by Hessling, as

* See Canstatt, Bd. i. p. 50. 1864. † Ibid. ‡ Zeitschr. für Wissensch. Zool., Bd. v. Heft 2.

well as by Kölliker, as being possibly pathological. WEBER mentions a case of hypertrophy of the tongue, in which, after amputation, a relapse occurred, and in the portion excised on the second occasion elementary materials resembling embryonic primitive fasciculi were found.

According to MAZONN, of Kiew, the contractile fibre cells of smooth muscle, described by Kölliker as existing in all four vertebrate classes, do not exist as such, but are only fragments of properly continuous fibres. He denies also the existence of the rod-like or columnar nucleus, founding his observations on muscular fibre macerated in dilute sulphuric acid.

The radiating or dilating fibres of the iris, according to Kölliker, consist of numerous slender fasciculi, uniting here and there in a net-like way, at sharp angles, which pass from the outer edge of the iris, and in an arched form pass into the sphincter, the fibres being often so arranged as to form regular arches. Other weaker fibres pass posterior to the sphincter, or between the fibres towards the pupillary margin, mostly without quite reaching it.

Nervous System: Nerves—Ganglion Cells.—GERLACH withdraws his former views (op. cit.), and now considers the axis-cylinder of the nerve to be a structure different from the nerve-marrow (white substance of Schwann). Among those nerves which are non-medullated, or marrowless, the olfactory nerve of man and the ox are to be ranged, according to KÖLLIKER. The latter establishes the existence of ganglia in renal nerves, only, however, in the main branches of the pelvis; never in the true renal substance in man and calves. In man the ganglia seem to be a-polar, whilst in the sheep numerous fine fibres take origin from them. Gerlach assures himself of the loop-like terminations of primitive fibres of nerves in the secondary fungiform papillæ of the tongue, and he is partly supported also by new examples on the part of Kölliker, who thought he detected these loops, as for instance in warts on the clitoris in man and the pig, and possibly in the iris. The supra-renal bodies were found by Kölliker to have large numbers of nerves distributed to them, nerves without any of the fibres of REMAK. These penetrated in all directions into the central parts, without entering the cortex, forming a very rich plexus, in the mammalia, in the medullary parts, but the method of termination of the nerves could not be ascertained. Ganglia existed on the nerves outside the organ.

Retina.—REMAK describes the structure of the retina at some length.* He states, that next to the "membrana limitans," extend, in the direction of the meridian of the eye-ball, the textural fibre fasciculi. In the interval are crowded thickly-placed multi-polar ganglion cells, which belong to the cell-layer existing on the outside of the optic nerve-layer. One may then discriminate on the inner surface of the retina fibrous and gangliated meridians. The branched projections of the ganglion-cells have the properties of nerve-fibres, and unite with the third very thick fibrous layer, from which exceedingly delicate varicose fibres, like the fasciculi of the optic nerve, run from behind forwards. On this fibrous layer follows again a layer consisting of small multi-polar ganglion cells, which in many places is separated by a very thin fibrous layer from the so-called granular layer. In the last one, neither nerve fibres nor ganglion cells appeared. It consisted of nuclei-holding fibres, arranged in a radial direction, which, on their extended nucleus-holding outer surface are beset with the well-known little rods and pegs. Projections of these fibres pass through the layer of the retina in a radial direction, and form, by means of broader thickly-crowded anastomosing enlargements, the "membrana limitans," without ever affording a resemblance to, or connexion with, the nerve fibres. These radiating fibres, discovered by H. Müller, appear, like the limiting membrane from which they proceed, to serve the retina as a support. In the district of the "yellow spot" there run, according to Remak, the layers of the retina, consisting of nerve fibres and ganglion cells in a gangliated

* Deutsche Klinik, No. 16: quoted from Canstatt. 1854.

sheet, formed by multi-polar ganglion cells, whose thinned, pitcher-shaped, central part (*Fovea optica*) in the sound eye of a boy, appeared to be perforated by a fissure. Up to this cavity a very thin projection of the inner fibrous layer passed, but here ceased the radiating fibres of Müller. The ganglion cells were surrounded by firm sheaths, from which proceeded the short stems of the pegs existing on the outer side. Between the above-named cavity and the choroid is a substance of deep yellow colour, clear as glass. In the other parts of the spot the stems of the pegs are very long and twisted, and, as it appeared, form a thick, very yielding cushion, upon which the gangliated lamina rested.

For KÖLLIKER'S observations on the extension and termination of the cochlear nerve of the ear, in which, to a certain extent, he agrees with Corti of Turin, see his *Histology*.

WAGNER* considers that all so-called a-polar cells do not exist as such, but that in all cases, excepting perhaps in those of the heart, about which there was doubt, they were mutilated bi- and multi-polar cells. He also considers that uni-polar cells in the brain and spinal cord are doubtful. Many apparently bi-polar cells in the brain he recognised as multi-polar, and he looks on the granular material between the cells as a bed for bloodvessels, and also as a matrix for the formation of new ganglion cells. The union of the ganglion cells described by him in the electrical apparatus of the torpedo, could not be found by REMAK, who asserts that the cells are all multi-polar, and that their processes partly enter into the roots of the electric nerves, and partly into the spinal cord. The cells in spinal ganglia were found by Remak to be bi-polar; but in the sympathetic ganglia they were multi-polar, with from three to twelve processes. They were more numerous in the solar plexus than in the terminal ganglia. In these ganglia the bi-polar cells differed from those in the spinal ganglia, by the branching of both processes, so that they were equivalent to multi-polar cells.

Spinal Cord and Nerves.—BIDDER observed the transition of primitive fibre cells of the roots of the spinal nerves, as also the longitudinal fibres of the white substance, into cells of the grey substance in great abundance.

In an Inaugural Dissertation before the University of Dorpat, by PHILLIP OWSJANNIKOW, the author has entered at great length on the structure of the spinal cord and spinal nerves. The conclusions drawn from the observations were as follows:—

1. That all the fibres of the spinal nerves which enter into the spinal cord are united to gangliated cells.
2. That one filament extends to each gangliated cell from the anterior spinal root, and one from the posterior root; a third, a commissural one, from the other part of the spinal marrow; and in many fishes a fourth, passing from the brain. The presence of this single fibre passing to the brain may, as the author throws out, be of moment in reference to the question of the possibility of the same fibre being both afferent and efferent in function, a position which DU BOIS-RAYMOND thinks tenable, as judging from his experiments, though, as a rule, this power is not put into action, in his opinion. The author of the paper now considered, thinks, on the contrary, this aforesaid power is always put into use.
3. That from each cell of the spinal marrow, a filament extends to the brain, forming the white substance.
4. That the chief mass of the spinal marrow, containing fibre and cells, is a united areolar web, which being arranged in great abundance about the central canal, and furnished with numerous bloodvessels, produces the ruddy grey colour of the substance which is generally supposed to be owing to pigment cells.
5. That the gelatinous substance of ROLANDI is connective tissue.
6. That the cells found as well in the posterior horns as in the surrounding substance of Rolandi, are corpuscles of the united web.

* Neurologische Untersuchung. See Canstatt, p. 61. 1854.

7. That the cylindrical axes are of a round form, and are composed of the same substance as the nervous cells.

8. That the cylindrical axes in the grey substance are formed of a membrane peculiar to themselves, which surrounding also the nerve cells, may be separated from the fundamental mass composed of the united web.

9. That in some fishes, the cylindrical axes of the spinal cord are exposed, the cellular web in which they are placed forming no special investment.

10. That in those fishes which have anterior and posterior spinal roots, round gangliated cells are found, sending out in various directions divided branches.

Very extensive observations have apparently been made on the question of the extent to which spinal ganglia are centres of nutrition for the sensitive nerves, by SCHIFF.* He arrives at conclusions differing from those of WALLER, reported on a previous occasion. He states as follows:—When the posterior root, between its origin and the ganglion, is divided, and the stump of the root remaining on the ganglion is short, then it becomes degenerated by inflammation. If the ganglion partook of the inflammation, then the entire nerve degenerated. But if the inflammation was only very slight, then the sensitive nerve remained normal, as well above as below the ganglion, in spite of the separation from the central part: the stump of the nerve connected with the spinal cord alone degenerating. In the motor nerves the case was reversed, but in all experiments with these nerves some small normal fibres still remained in the degenerated nerves, and to these corresponded single degenerated fibres in the not otherwise degenerated stump of the motor nerve connected with the spinal marrow. Schiff considers these fibres as returning from the undegenerated sensitive nerves, from which the anterior spinal roots obtain their sensibility. He found only degenerated fibres in the muscles; and thus Waller's view, that no nerve passed into these structures from the posterior root, seemed to be confirmed.

According to WAGNER, the grey commissures before and behind the spinal central canal, are only transverse commissures for the ganglion cells. He states also that he can classify the primitive fibres of the brain in five sets, according to their size. They all have axis cylinders, and union between them exists only by intervention of ganglion cells.

Pacinian Bodies.—Pacinian bodies were observed in the clitoris of the swine by NYLANDER and KÖLLIKER. They had previously been observed by FRICK on the glans penis. They have been found by LEYDIG in the interspaces of the bones of the forearm of the mouse, being $0.72'''$ long; their central space was filled with pale granular substance.

Uterine Organs, &c.—The amnion of the human fœtus is described by REMAK† as consisting of a thick layer of striped connective tissue, which, at distances of about $\frac{1}{25}'''$, contained foramina of $\frac{1}{50}'''$ in size, and of a granular cell-layer; and between these two layers existed tubular or stellate cells, with one or more nuclei, forming a network with very wide interspaces.

Cartilage and Bone.—According to FORSTER,‡ normal cartilage increases internally by an endogenous increase of the cells, externally by a transformation of connective into cartilage tissue. This transformation does not quite accord with the plan suggested by VIRCHOW, inasmuch as the fibrous blastema of the perichondrium is transformed into homogeneous cartilage substance, the connective-tissue corpuscles increasing and becoming surrounded by a clear glistening capsule. The ray-like processes of the cartilage cells which become bone canals, belong, in his opinion, to endogenous cells; they grow during ossification, gradually pierce the thick capsule wall, and unite with the projections of other cells.

REDFERN does not recognise the measurements made by Toyneec and Carpenter, indicating that the thickness of articular cartilage diminishes as age advances.

* Arch. des Vereins für gemeinschaft. Arb., Band i. Heft 4.

† Müller's Archiv, Heft 4, p. 369. 1854.

‡ See Canstatt, p. 67. 1854.

KÖLLIKER also is opposed to the distinction made by Bruch between cartilaginous pre-formed bone, and bone formed out of secondary blastema; for the description of which, see our April number last.

Glands and Ducts: Stomach and Intestinal Glands.—GERLACH, like Donders, agrees with Bruch in considering Peyer's glands as analogous to lymphatic glands. No clustered glands were found by SCHIAPPEL* in the mucous membrane of the human stomach; and he looks upon those found by Handfield Jones in the pyloric wall as pathological, and owing to partial destruction of single tube membranes, and the massing together and union of the débris.

Sweat Glands.—These were found, like those in the axilla, by SAPPY, in the walls of the anterior and lateral parts of the thorax. The difference made by KÖLLIKER between the excretory ducts of the large and small sweat glands is opposed by TOBIEN. The latter perceived, external to the epithelium of all gland ducts, a structureless layer, beset with elongated oval nuclei, which he considers to be identical with the layer looked on by Kölliker as muscular. The ducts of large axillary sweat glands were not always found by DONDERS to contain fibre cells; but he always met with a structureless membrane, having at first sight a fibrous look, but which, on treatment with acetic acid or alkali, was seen to be homogeneous, without trace of nuclei†

Salivary and Lachrymal Glands.—According to TOBIEN, great differences existed in the structure of Steno's duct. In an old soldier, two muscular layers were found, the inner one longitudinal, the outer stronger and annular. In the bodies of five young men, an elastic fibre network in four layers took the place of three muscular layers in Steno's duct, alternating in a longitudinal and circular direction, the outer one being longitudinal.

Wharton's duct possessed two elastic layers under its epithelium, the outer one having a circular course; and the pancreatic duct consisted of areolar tissue, including two layers of elastic fibres, the inner longitudinal, the outer circular.

Seminal Glands, &c.—The seminal canals were found by KÖLLIKER not to contain any muscular fibre, and at the upper part of the epididymis there were only short fibre cells with short nuclei.

Lymphatic Glands.—These glands have been examined and described by BECK ‡ His results differ somewhat from received views. He found that the coverings of the glands consisted of strong areolar tissue and much elastic fibre, which passing in all directions into the gland, formed a firm stroma, with numerous interstices or alveoli. He found not any muscle fibre in the covering. Between the fibres of the stroma, in the closest proximity to the absorbent vessels, ran the blood-vessels, which divided into many branches, forming a rich network of vessels around the absorbent glands and alveoli. No direct communication was found to exist between the veins and the absorbents. The spaces which give to the gland a vesicular appearance, contained no special fluid or granules, no pulp or parenchyma, but only elementary lymph corpuscles. The absorbent vessels around the capillary vessels divided freely, formed projections and pouches, which were filled with lymph cells. Beck also found lymph vessels, taking their origin in the gland, evidently in connexion with the alveolar network, the thinning of the vessels' walls being quite gradual. Beck agrees with Heyfelder in thinking that no special parenchyma, no free cell-spaces, existed in the lymphatic glands; but that meshes of the stroma were full of absorbent vessels. He thought the round or somewhat oval bodies in the interior of the vessels, and partly in the canals between the fibres, were merely true lymph corpuscles. In his opinion, the lymphatic glands might be defined to be simply a network of well-protected blood-laved absorbents, the starting points of new lymphatic vessels and lymph corpuscles. He compares

* Arch. für Path. Anat. und Phys.

† See Oanstatt, p. 71. 1854.

‡ Illustrirte Medizinische Zeitung, Heft 6.

their structure to that of the spleen, in which he found no special parenchyma, as did not Bennett and Schultze.

The above description—which is not altogether very complete, inasmuch as the mention of certain important points is omitted, such as the exact relation between the afferent and efferent vessels—will be seen to differ materially from that accepted by Kölliker in several particulars. Beck seems to consider that the morphological elements within the alveoli of the stroma are only elementary lymph corpuscles, as it would seem, formed in the glands. KÖLLIKER, on the other hand, considers that there is a definite parenchyma or gland substance, and that the cell elements, &c., have no direct connexion with the lymph as lymphatic corpuscles. He speaks also of the alveoli containing a fine vascular network—a fact not mentioned by Beck. They seem to agree, however, to a certain extent, in considering the alveoli as a modified part of the lymphatic vessels going in and out of the glands. Kölliker had not met with the muscular tissue in the covering and septa of these glands, as had Beck and Heyfelder.

Liver.—The minute anatomy of this organ has been lately examined by BEALE,* who, from his dissections and injections, comes to the following results:

1. That the essential constitution of the liver is that of a double network of minute vessels, one of capillary bloodvessels, and another of cell-containing tubes, naturally adapted to each other. Both of these sets of tubes in each lobule appear to communicate with those of the neighbouring lobules in all livers excepting that of the pig; and this circumstance is connected with the fact, that in all other animals but the pig the hepatic lobules are not isolated by intervening and limiting fibrous tissue or capsules. As to the latter position, Beale agrees with Weber.

2. That the cell-containing tubes are in all vertebrata continuous with the ultimate fine ducts of the viscus; in some cases directly so, whilst in others, as in the rabbit, and slightly in man and the dog, a fine network of the ducts themselves intervenes. The basement membrane of these tubes being, after foetal life, incorporated with those of the capillaries, so that the secreting hepatic cells are only separated from the stream of blood by a single intervening membrane. The cell tubules contain the hepatic cells, as also granular and colouring matter and cell debris; the cells observing no order of arrangement, as some have thought, and contrasting in size, &c., greatly with the epithelium lining the ducts, from which they are strictly separated.

3. That the fine ducts are many times narrower at the point where they are continuous with the cell tubes, than those tubes themselves; and that the larger ducts and larger interlobular ducts freely anastomose with each other.

4. That whilst the finest biliary ducts are only composed of basement membrane, that of the larger ones is more complex, containing numerous cavities; especially in the pig, which, although generally considered to be glands, are in fact reservoirs for the bile, retaining it, and bringing it into intimate relation with the abundant surrounding bloodvessels, so that it may undergo requisite changes. This the author also considers to be the function of the vasa aberrantia, so named by Weber.

In this view it will be seen that Beale considers the structure of the liver to be strikingly different from that described by Kölliker and Handfield Jones, and assigns a different office to the secreting and epithelium cells; for whilst the latter looks upon the cells of the ducts as chiefly forming the bile, Beale considers that they stand in relation to the hepatic cells as the columnar epithelium (lining the stomach tubes) does to the secreting cells at the bottom of them.

Beale prepared his specimens by injecting the portal vein with lukewarm water until the bile was washed out of the ducts by it, and then injecting the ducts; after which the portal vein was injected with size. The ducts were also examined

* Proceedings of Royal Society, June, 1855.

in specimens hardened in alcohol, to which a solution of soda had been added, in order to render the sections transparent.

DUSCH* finds that the hepatic cells are dissolved in bile and in solutions of glycocholate of soda. They also enlarge on the addition of chloroform, according to LEBEBOULLET, their contents becoming very clear.

Vascular Glands.—The Spleen.—This organ has been examined at some length by FUEHRER,† who came to the following results. He states that a fine parenchyma did not exist, but the whole spongy tissue was composed of arterial and venous vessels, along with a very remarkable capillary network of so-called capillary cells. The capillary cells are nothing else than the well-known chiefly crescentic fibre cells, with large bulging nuclei, looked upon formerly as muscular, but considered by Fuhrer as the special seats of blood formation. These cells were considered by the author to be very like the free blood-corpuscles floating around. At one time they were smooth and homogenous; at another, finely granular, and often slightly-coloured yellowish-red. On the other hand, in the splenic blood there were blood-corpuscles, which were provided with a single or double appendage, just like those nuclei after they were squeezed out of the cells. There were found also, especially in spleen rendered spongy by exudation, connecting networks of these crescentic cells, unless the isolated ones were separated by a special sheath-wall, so that they formed in consequence a branched tubular system, in which these nuclei lay disposed singly. Fuhrer noticed the continuous connexion of such capillary-cell network with clear bloodvessels—the nuclei-holding tubes becoming capillaries, which contain young pale blood-corpuscles at various distances—that is, the nuclei of the so-called crescentic fibre cells. Sometimes the nucleus fails in the above cells, and it is only indicated by a spindle-shaped extension of the cell, accompanied by a certain cloudiness. The capillary processes, as Fuhrer calls the projections of the cells, are often so bent towards each other that a completely annular shape is assumed by them. He does not corroborate KÖLLIKER's statement that such curved cells are imbedded in a cell membrane. On the contrary, he sees in the endeavour of the bodies in question to roll themselves into an isolated state, and in their wavy contour, a special contractility, and an indication of their arterial vascular character. Their ready solubility in acetic acid seems to show that it is not merely elastic membrane which forms the tubuli and cell-walls. According to Fuhrer, these bodies are equally extended over all parts of the spleen, only in the spleen-corpuscles they form a much finer network than in the pulp. Fuhrer thinks he distinguishes younger and older capillary cells, inasmuch as those which show only an extension in the place of a nucleus, having sharp-pointed projections, are most probably less developed. Hence, in a more abundant ramification of the capillary cells, quite the peripheric extensions are in this way much diminished in size. In spleens also, with a presumably quickened blood-formation, that is, in the embryotic spleen, very large numbers of such singly-caudate, non-nucleated, as also disjointed capillary cells, exist. Fuhrer then proceeds to point out the primitive origin of these.

The above-described special tubular system Fuhrer considers to be the only capillary vascular system in the spleen, but one in its single parts transient, and ephemeral. All those free granular elements which appear on rupture of the spleen he takes to be only blood and nuclei of the capillary cells, separated and pressed out of the finer vessels with the splenic fluid, in various stages of development. All larger cell elements belong not to the normal spleen parenchyma, but are colourless blood-corpuscles of various kinds, poured along with the blood out of larger vessels. Free granular pigment also exists in each spleen, more or less abundant; but blood corpuscle-holding cells, which Kölliker observed, and which some repudiate, are considered by Fuhrer to be rare.

• The veins of the spleen were found by him, as by GRAY, to possess an enormously wide calibre immediately after their appearance. According to Fuhrer,

* See Canstatt.

† Arch. für Phys. Heilk., Heft 2, p. 155. Canstatt, p. 72. 1854.

they arise from the coalescence of those capillary cell networks in which the young blood-corpuscles lie in rows, the more closely the nearer to the entrance into the vein. The arterial ramification, on the contrary, is very fine, gradually passing into the capillary network. Our author looks upon the *Malpighian bodies* as consisting of the finest arterial branches, with a crown of capillary cells, very fine and thickly laid. He finds no veins within them, and the arterial branches are so fine as mostly not to admit red blood-corpuscles; hence their white colour. He failed to obtain indication of the presence of any investing membrane of the Malpighian corpuscle, excepting the sheaths of the bloodvessels; and generally there was no clear limit between the corpuscle and the red spleen-pulp. The above views will be seen to differ materially from those of Kölliker, Günsburg, and Gray. With regard to the observations of the latter observer, it may be well here to notice one or two points which were not commented on in the notice of them given some time ago in this periodical. Thus we find that by him the *smaller veins of the spleen* are considered to have an important relation not only to the pulp, but also to the Malpighian bodies. Each of these bodies seems, he states, to be enclosed completely by an imperfect capsule, formed of the small primary veins. These vessels, of large size, commence on the surface of each Malpighian body throughout the whole of its circumference, and radiating from the central part, join with similar branches, either on the surface or towards the circumference; lastly, these larger veins empty themselves into the neighbouring ones of the pulp. The smaller veins appear to act in carrying off the secretion formed in the Malpighian bodies into the circulation.*

MAZONN states most clearly that the covering and trabecular tissue of the human spleen contains muscular fibre.† This was neither found to be the case by Gray, Günsburg, nor Kölliker. The latter found it, however, in some of the lower animals.

III. REGENERATION OF PARTS.

Epithelium.—GERLACH‡ and HESSLER§ assert the existence of free nuclei in the lowest layers of laminated integument. KÖLLIKER, DONDERS, and HARTING assert that the complete cells take a part in the formation of the new ones. Kölliker saw the nuclei in the epithelium cells of the tunica vaginalis to contain two nucleoli, and to divide into two nuclei; and among the epithelium of the urinary bladder the frequent occurrence of two nuclei in one vesicle, which, along with Virchow, he saw, proves to him an endogenous cell production.

HENLE, however, thinks that an undoubted instance of endogenous cell-formation in the adult human body is wanting, and is necessary before any value can be placed on such fragmentary and ambiguous facts.

According to DONDERS,|| the cylindrical cells of the mucous layer of epithelium are to be looked upon as the locale for the proper newly-formed cells. He thinks they may be, towards the interior, united with the cutis as with the rounded cells of the mucous layer external to them, and withstand the influence of alkali longer than the last. In this case Donders thinks they may be older than the cells of the more superficial stratum, and probably not destined to move outwards.

* Gray on the Structure and Use of Spleen, p. 131.

† Müller's Archiv, No. 1, p. 25. 1854.

‡ Handbuch der Gewebelehre, &c.

§ Quoted from Canstatt.

|| Handleiding voor de Naturkunde van den gezond. Mensch.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By EDWARD H. SIEVEKING, M.D.

1. *On the Nature and Treatment of Encephalo-Meningitis.* By M. LÉON LIÉGARD, D.M.P. (Revue Médico-Chirurg. de Paris, Jan. et Févr., 1855.)

DR. LIÉGARD, whose special attention was drawn to the subject of the above-named memoir by the excessive fatality of the meningitis of children in the Hôpital de l'Enfant Jésus, and by the apparent inefficacy of the various remedies of an opposite character administered, supports M. Robin's opinion, that the granulations met with in these cases are not of a tuberculous character, and that we must therefore materially modify our views with regard to the nature and treatment of the disease. It appears that the medical officers of the hospital had been so discouraged by the inutilty of applying either the antiphlogistic, the alterative, or the tonic mode of treatment, that they abandoned therapeutic attempts altogether, and, a diagnosis of meningitis having been established, resigned the patients to their fate. The views advocated by the author are mainly based upon Robin's micrographic account of the granulations, so that it is necessary to give a summary of his description.

Robin assumes two varieties. The first is of a yellowish tint, softish to the touch, and friable. Nine-tenths of these granulations consist of a finely granular amorphous matter; they contain a small quantity of cytoblasts, a few capillaries, and occasionally fibro-plastic formations. The second may, and most frequently does, co-exist with the former in the meninges, and is also found coincidently in other serous membranes. It is the form specially known under the term *grey* or *semi-transparent granulation*. Robin finds that the bulk of the product is composed of the same amorphous granular matter as that above described, but that it is firmer; the cytoblasts are in large numbers, as is particularly manifest on the addition of acetic acid. Fibro-plastic elements, and a network of fine areolar tissue, accompanied by scanty vessels, also present. These cytoblasts are described as differing from the tubercle corpuscles (1) in being polyhedric, with irregular and slightly-toothed margins, instead of oval or round; (2) their diameter averages half the size of the latter; (3) the tubercle corpuscle is rendered pale by acetic acid, but is not dissolved; the contour of the cytoblasts is rendered more defined; (4) the granulations contained in the tubercle corpuscle are scattered uniformly through the corpuscle, while in the cytoblasts they are congregated in the middle; (5) although the cytoblasts contain no nucleolus, they belong to the cellular element which commonly exhibits the presence of free nuclei. We scarcely ever find the variety of free nuclei without meeting with some cells containing a nucleus resembling the free nuclei.

Assuming this definition of M. Robin's to be correct, the author regards the inflammation which determines the formation of the granulations to be of a specific character, analogous to that of croup, and attributes the fatality of the disease in the children's hospital to the erroneous assumption that it was of a tuberculous character. M. Liégard then quotes several cases diagnosed as tubercular or granular meningitis, in which cures were obtained by powerful antiphlogistic remedies, especially by venesection or leeches to the mastoid processes, and mercurial inunctions over the abdomen, repeated every three hours, with calomel administered internally. The author admits the impossibility of diagnosing the granular from the simple form of meningitis, and proposes the classification of the disease, according to the violence of the symptoms and the rapidity of its course, into the inflammatory and sub-inflammatory forms. According to the intensity of symptoms, he recommends a variation of the treatment, not in kind, but in degree; and he concludes, both from the character of the morbid changes and the results obtained on the basis of the views advocated, that this fearful malady may be successfully combated by the means proposed, and ought not to be regarded as necessarily fatal.

2. *A Contribution to the Doctrine of so-called Paralysis Musculaire Progressive.* By Dr. THEODOR VALENTINER. (Vierteljahrsschrift für die Praktische Heilkunde, xii. Jahrg. 1855, p. 1.)

THE term paralysis, or atrophic musculaire progressive, was applied by Cruveilhier to cases of fatty degeneration of the muscles, resembling those detailed by Dr. Meryon,* in which no essential morbid change could be traced in the nervous centres;† but in which the gradual and entire loss of power of certain sets of muscles, accompanied, as Duchenne had pointed out, by loss of electric excitability, constituted the main symptoms, while both naked-eye and microscopic examination demonstrated a complete fatty degeneration of the affected muscles. Dr. VALENTINER'S case is of considerable interest; we can only give it in a much abridged form. It will be seen that the primary lesion in this instance appears to have been in the spinal cord.

A gentleman, aged 45, of robust and athletic habit, and peculiarly gifted, had always enjoyed good health; and although he had occasionally committed excesses in his cups, was generally temperate, and remarkably fond of gymnastics. Ten years previously he once fell on his back, on a sandy soil, from a height of eight or ten feet. About two years after this, the patient, who had experienced no inconvenience at the time of the fall, thought his health failed; his florid complexion became sallow. A year later, in 1847, he had a slight attack of pleurisy; but as late as 1852, he could have experienced no great diminution of physical strength, for he was still able to carry two fifty-pound weights in each hand. In April, 1853, he first consulted Dr. W. H. Valentiner, who found that he had then very little power in his hands; that he was unable to exert any forcible compression, or stretch out his fingers completely; the right hand was the weaker of the two; no emaciation was perceptible; there was a difficulty about all the movements of the body, and the patient had a difficulty in conveying his food to his mouth; no tenderness or change of any kind was perceptible in the spine; no cerebral affections could be traced. The various physicians consulted, regarded the affection as one of the nervous system, without being able to localize it. The patient was sent to Franzensbad, a Bohemian watering-place, where he used the baths, and then went to Nancy, in the south of France. Dr. T. Valentiner (the author) was now consulted, and having become acquainted with Cruveilhier's cases, diagnosed the present one as an instance of fatty degeneration of the muscles, with atrophy of the anterior roots of the spinal nerves. In the autumn of 1853, though the paralysis became more and more marked, some hopes were excited in the patient by the occasional occurrence of sudden and frequent twitches in the affected muscles. In November, the patient was conveyed back to his home; he then exhibited extreme emaciation of the upper extremities; none of the limbs could be properly extended; in an attempt at walking, the feet dragged on the ground; his back was almost bent double; the face, though already showing symptoms of paralysis, still retained its intellectual expression; sensibility continued unimpaired in all parts of the body. At last, dysphagia supervened, and although the appetite continued good, the paralysis progressed, and a slight attack of bronchitis, in March, 1854, terminated the patient's misery.

The following are the main results of the post-mortem examination:—The deltoids and other muscles of the upper and fore-arm had almost disappeared; the muscles of the hands were entirely converted into fat; the muscles of the cervical and lumbar region presented a tolerably healthy appearance, but in the dorsal region they were pale, and traversed by yellow bands; and one fasciculus closely resembled fat. The microscope confirmed the various degrees of fatty degeneration observed by the naked eye. The muscles of the lower extremities were in a healthier state than those of the upper. On sitting up the entire dura mater of

* Medico-Chirurgical Transactions, vol. xxxv p. 73.

† In one of three cases detailed by Cruveilhier there was atrophy of the anterior roots of the spinal nerves.

the spinal cord, about one hundred small white bodies were observed scattered over the dorsal surface of the cord, exclusive of the cervical portion; varying in size from a pin's head to a small pea, smooth externally, rough on the inner side, and grating under the knife; under the microscope they exhibited a dense fibrous tissue, interspersed with pigment. All the anterior roots of the spinal nerves were distinctly thinner and smaller than the posterior roots, flabby, resembling a tissue filled with a reddish serum, and exhibiting under the lens a marked vascularity; healthy nerve-fibres were still visible under the microscope, but many were in a state of fatty degeneration. Nothing of the kind was found in the posterior roots; the cord was found abnormally soft at the part where the three lower cervical and four upper dorsal nerves are given off; at this part the distinction between the grey and white matter was almost effaced; numerous glomeruli (granular corpuscles) were found in the softened part; they occurred in the white, as well as in the grey, substance. Scarcely any well-marked ganglionic cells were discoverable in these parts, but were found in the unsoftened portions lower down; no marked lesion could be discovered in the brain; the heart, healthy to the naked eye, showed under the microscope incipient fatty degeneration; much oily matter was contained in the hepatic cells; the nerves of the brachial plexus exhibited no abnormalities; excepting congestion of some parts of the lungs, and slight pleuritic adhesions at the right apex, no other visceral lesion was discovered.

The author of the paper is of opinion, and it would appear that he is fully borne out in the view, that chronic arachnitis, first caused by the fall, was the fundamental lesion in the case just detailed, and that the atrophy of the nerves and the fatty degeneration of the muscles were consecutive. It is curious that, considering the site of the arachnoideal formations, there should have been no lesion of sensibility.

3. *Three Observations of Adherent Pericardium.* By Professor CEJKA. (Vierteljahrsschrift für die Praktische Heilkunde, xii. Jahrg. 1855, p. 128.)

IN each of the cases related by Professor CEJKA the diagnosis of complete adhesion of the two laminae of the pericardium was established during life, and confirmed by the post-mortem. In each there was adhesion between the pericardium and the costal pleura. The characteristic symptom in each case was the depression of one or more intercostal spaces over the heart at the time of the systole. The dulness over the heart was extended, and the sounds only feebly audible. The cases confirm Skoda's views on the subject, for which we may refer the reader to Dr. Markham's translation, p. 327.

4. *Remarks on some of the Anatomical Conditions that favour the Transmission of Sounds from the Root of the Bronchi to a Distant Part of the Thorax.* By E. BARTHEZ, M.D. (L'Union Médicale, June 7th, 1855.)

A CONSIDERABLE difficulty presents itself at times to the auscultator in the interpretation of what Skoda has termed consonating sounds. Dr. BARTHEZ evidently adheres to the theory of the direct conduction by the tissues intervening between the part at which the sound is produced, whether a respiratory murmur or a bubbling or other noise, and the point to which the observer's ear is applied. The following case affords a striking illustration of the physical circumstances attending the production of caverno-amphoric respiration in pleural effusions, and appears to us to be more readily explicable by the theory of consonance than of conduction.

Dr. Barthez was requested by M. Marjolin to see a child, aged 5 or 6 years, affected with a purulent effusion into the pleura, which had opened externally near the nipple by sinuses, which were so extensive as to prevent the air from entering the thoracic cavity. The discharge of pus having been arrested for some days by the dressing, the presence of a certain quantity of liquid in the pleura was proved

by general dulness, and bronchial respiration throughout the (upper part of the) thorax posteriorly. Under the clavicle the respiration was manifestly cavernous. On the introduction of a probe into the thorax, carried along the sinus, a continuous jet of pus escaped. While this occurred, the bronchial respiration disappeared gradually at the pulmonary apex posteriorly, to make way for a feeble but normal respiratory murmur in and below the fossa infra-spinosa. Immediately after the evacuation of the pus, and while Dr. Barthez applied his ear to the thorax, tepid water was injected at intervals into the pleura. The injection was effected without the production of any sound, and the thorax was filled without any perceptible phenomena, until at a given moment the bronchial respiration of the infra-spinous fossa assumed the cavernous and even amphoric pitch, which several of the gentlemen present were able to confirm. Afterwards, in proportion as the injected fluid was evacuated, the amphoric respiration disappeared in order to make room for a bronchial blowing, similar to that previously existing. Finally, a powerful inspiration having caused the admission of some air, metallic tinkling became very evident, which was repeated each time a fresh injection was made. From that time, in spite of the injections, the amphoric breathing was not reproduced. The child subsequently recovered, and the normal respiratory murmur was restored on the affected side.

The last circumstance adverted to, the impossibility of reproducing the amphoric sound after the admission of air, is strongly corroborative of the law of acoustics, that the consonance of sound is dependent upon the uniformity of the media through which it is transmitted.

5. *Investigations regarding the Formation of Cavities in Tuberculous Lungs.* By Dr. HUGO RÜHLE. (Inaugural Dissertation, Breslau, 1853.)

THE prevailing opinion with regard to the mode in which cavities form in tuberculous lungs is, that after the deposition of the morbid product, secondary ulcerative destruction takes place, by which a breaking up of the pulmonary tissue is effected. Dr. Rühle analyses ten cases of phthisis pulmonalis, in which cavities were found, and concludes that they take their origin in dilatation of the bronchi; and that the ulcerative fusion of the parenchyma surrounding a tubercular deposit is, in the majority of instances, preceded by bronchiectasis. He finds that in proportion as the cavities diminish in size, the more unable we are to discover any limits between the mucous membrane of the bronchus leading into a cavity, and the membrane lining that cavity. The author is of opinion that the microscopic appearances of the membrane are not compatible with the view of its adventitious character. Moreover, the relation of the bronchus to the cavity is regarded as corroborative of Dr. Rühle's doctrine:—"The cavities are always in direct communication with the bronchi, and only one bronchus opens into each of the cavities here alluded to, and the communication is not on one side, but the axis of the bronchus coincides with that of the cavity." The author does not inform us at what time the ulcerative process commences, but states generally that it ensues early, and that, although non-tubercular bronchiectasis may be accompanied by ulceration, the tubercular deposit possesses a peculiar power of exciting the ulcerative process in dilated bronchi.

6. *Reduplication of both Sounds of the Heart.* By AUSTIN FLINT, M.D. (The Western Journal of Medicine and Surgery, April, 1855. Louisville, Kentucky.)

THE occurrence of a reduplication of one of the heart's sounds, especially of the first, though not very frequent, is one that most clinical observers have met with. A reduplication of both sounds is so rare, that the following case, which may be regarded as almost unique in the regularity of the phenomena and the completeness of the observation, deserves especial attention.

George Nash, aged 27, boatman, admitted into Louisville Hospital, December 16th, 1853. Excepting an attack of cholera eighteen months previously, he had enjoyed good health till seized with cold and cough six weeks before admission. Cough chiefly at night; soreness under sternum; no hæmoptysis, chills, or loss of appetite; bowels regular; never had rheumatism or acute thoracic affection. On admission: aspect not morbid, pulse normal, respiration slightly increased in frequency, skin and tongue normal, appetite good, bowels regular, no thirst, percussion of thorax clear, breathing movements equal on both sides, sibilant inspiration on both sides. Some dyspnoea for a few days, with palpitation from commencement of attack, requiring him to lie with his head raised. The treatment consisted at first in the administration of syrups of ipecacuanha, squills, and tolu, with a little sulphate of morphia. On December 23rd, seven days after admission, the pulse at the wrist was found too quick to be counted, "but the carotids can be felt and, enumerating in this situation, counting at the same time the heart-sounds, the number of ventricular contractions per minute is one hundred and sixty." No œdema or ascites. Chest well developed. No apex impulse of heart; a feeble diffused impulse felt just below the nipple, and both seen and felt at the epigastrium; no heaving of the chest; the heart-sounds succeed each other so rapidly that it is difficult to state them, but they appear pure, and the first sound is shorter than natural. On December 24th, the absence of pulsation in the jugular veins is noted. On December 28th, Dr. Flint says: "On examination of this patient yesterday, I found a feeble vibratory pulse at the wrist, numbering eighty per minute. I counted it repeatedly, with similar results; on counting the heart-sounds, I found them apparently one hundred and sixty-six beats—i.e., tic-tacs, per minute. I repeated the comparison of the heart-sounds several times, with the same results. To-day I find the same contrast—viz., pulse eighty, and two heart-sounds repeated one hundred and sixty times per minute. Dr. Dickinson counted, without knowing the results of my enumerations, with similar results." The same ratio was observed by Dr. Hardin, on December 31st; by Dr. John Clark, on January 1st, and also on January 3rd and 13th. On the 3rd of January, a faint, short, sharp bruit was noted below the pectoral muscle, with the systole at the right and left apex, but not at the base. On the 13th, a bruit, supposed to be endocardial, accompanied the systole, uniformly higher in pitch than the whispered word *who*, and nearly as high as *R*. On January 25th, œdema supervened, and the dyspnoea became more urgent. The bitartrate of potash, followed by the exhibition of Epsom salts, relieved the dropsy; and on February 14th the patient was discharged, feeling quite well; able to sleep in the recumbent posture, without dyspnoea on taking exercise; no cough, pain, or palpitation. "The pulse and the two sounds of the heart are eighty-four per minute. Marked dulness on percussion was observed to the left of the nipple. No point of apex impulse was seen or felt, but a very feeble impulse is appreciable over an area about two inches in diameter. No bruit heard." He left the hospital, and undertook severe work, without experiencing any inconvenience; and when Dr. Flint saw him again, on April 12th, the pulse at the wrist, and the ventricular contractions, were eighty-four each per minute; the sounds of the heart were pure, no apex impulse was appreciable, the dulness on percussion extended an inch to the left of the nipple, and the general health was good. He was again seen by Dr. Flint in February, 1855, when he continued well, and no physical evidences of heart disease were found, save that the area of dulness was somewhat increased.

In considering the causes of the phenomena in this case, Dr. Flint suggests two theories—either that the heart beat in the usual manner, but that, from the weakness of every alternate beat, that was not represented by a corresponding dilatation of the radial artery; or that the reduplication was owing to a want of synchronism between the contraction of each ventricle; each ventricle, as it were, asserting its independence by separate action. Want of synchronism between the ventricles is the cause to which Dr. Williams and Skoda attribute the reduplication of the first sound alone, and it is difficult to assume any other view in reference to Dr. Flint's

case; he himself does not express himself decidedly in favour of one or the other, although he inclines to the theory of synchronism. He remarks, that a fact which would be incompatible with it was, that the beats in the carotid artery were equal to that of the heart. "It is indeed wonderful," he says, "that of the halved portions of the systolic contraction of the left ventricle, only one should regularly be accompanied by a radial pulse, but it is, perhaps, quite as difficult to conceive that, in view of the arrangement of the muscular fibres of the heart, the ventricles should contract separately." The only point that suggests itself is as to the possibility of the peculiarity of the heart's action, whether explicable on the one view or the other, having been determined in the first instance by the remedies employed, and especially by the sulphate of morphia.

7. *Elephantiasis Orientalis*. By ALLAN WEBB, M.D., Bengal Medical Service. (The Indian Annals of Medical Science, No. 1, April, 1855.)

THE main object of this valuable communication is to determine the nature of elephantiasis by post-mortem examinations, for which the opportunities are not often offered. Dr. WEBB states that he has for ten years operated upon elephantoid tumours, without once having an opportunity of examining after death the state of the internal organs in cases of elephantiasis genitalis. The conclusions that he arrives at from the post-mortems which he has made (and of which we shall give a summary), as well as from observation of the vital relations of the disease, are, that it is, in truth, a blood-disease; that it affects the limbs, the subcutaneous cellular tissue, the sheath of the nerves; that it is metastatic, and effects organic changes in the heart, liver, spleen, kidneys, testes, and probably the brain. Dr. Webb sees an analogy between elephantiasis and rheumatism, on account of the first-mentioned features. In regard to the microscopic characters of elephantiasis, Dr. Webb agrees with Mr. Paget in describing it as essentially consisting of a fibrous outgrowth from an albuminous blastema, mingled with elastic tissue, and more or less fat. Professor Macnamara has found smooth unstripped muscular fibres in the tumours that result from elephantiasis of the scrotum, to the presence of which the author attributes the contractions which he has repeatedly seen ensue in the tumours when pricked. Dr. Webb absolutely denies that the disease is ever the result of an inflammation of the veins or lymphatics; at the same time, he regards all the varieties which are met with as essentially the same disease, the main difference appearing to consist in the rapidity with which it is developed, and with which it runs its course. While our author denies that the disease originates in inflammation of the veins or lymphatics, he appears to agree with Dr. Skinner that it has its origin in inflammation of the cellular tissue, into whose stretched and enlarged archæ a fluid is poured out capable of speedy organization. We do not find that the limitation to the left side, observed by Dr. Rigler,* is confirmed by Dr. Webb; he does not allude to the point, but in the cases which he gives, the right side appears to have been most frequently attacked: in one, the left leg was first attacked, then the right arm, and lastly the scrotum; nor does there seem to be any definite relation between an affection of the lymphatic glands and the occurrence of elephantiasis, as suggested by Dr. Rigler.

The following is a brief summary of the post-mortem appearances found in the necropsies of cases of elephantiasis: from them it would follow, as Dr. Webb points out, that fatty degenerations of various organs are concomitant affections with elephantiasis. Whether they are merely a secondary result of the change of nutrition produced by the drain upon the constitution by the morbid deposit in the scrotum, or extremity, or whether the disorganizations of the viscera are primary affections, must be determined by further investigations.

* See British and Foreign Medico-Chirurgical Review, July, 1855, p. 246.

CASE I.—Man, aged 30. Elephantiasis scroti; duration two years; death from sloughing of scrotum. Fatty degeneration of heart; liver fatty, with old adhesions of right lobe; spleen four times its natural size; kidneys three times their normal size, internally softened and yellow, with a few cysts.

CASE II.—Man, aged 30. Elephantiasis of right leg; duration about four years. Amputation; death. Universal effusion between arachnoid and pia mater; liver and heart showed fatty degeneration under microscope.

CASE III.—Man, aged 50. Elephantiasis scroti about ten years; of right leg about two years. Death from suppuration and sloughing of leg. Fatty degeneration of heart; thick flakes of fibrin on right auricle; atheroma in descending aorta, and absolute obliteration of aorta about one inch below the coeliac artery; liver cirrhotic, weighing two and a half pounds; spleen softened, three times its normal size; kidneys encysted, and with fatty degeneration.

CASE IV.—Man, aged 40. Elephantiasis of scrotum and right leg and foot, preceded by swelling of inguinal glands of same side; duration fifteen years; removal of scrotal tumour; death from diarrhoea, dysentery, and sloughing. Liver pale, spotted with yellow patches, apparently from fatty degeneration; spleen double the normal size, with thickening of capsule. Fatty degeneration of epithelium of kidneys; gangrenous mass in posterior part of right lung; right side of heart of pale brown, marked fatty degeneration throughout; aorta covered with atheromatous deposit.

CASE V.—Man, aged 10. Elephantiasis scroti; duration seven years; operation; death three weeks after, with fever and delirium; liver cirrhotic, the colour of the pancreas; inferior vena distended immediately under liver, completely obliterated close to the heart; capsule of right kidney adherent; cysts in kidneys; left kidney enlarged and pale; spleen much enlarged; apparent fatty degeneration of heart; considerable serous effusion upon brain and in ventricles; brain tissue hardened.

CASE VI.—Elephantiasis scroti; duration eight years; right side of scrotum first affected. Operation; death. Lungs oedematous; heart fibres had in places almost entirely disappeared, and fat substituted within them; kidneys, and liver, and testes showed fatty degeneration.

CASE VII.—Man, aged 10. Elephantiasis of penis and scrotum; duration seven years; operation; jaundice; delirium; death. Effusion of serum, blistering the arachnoid, and causing compression of medulla to the size of a little finger; spots of apoplexy in lungs; heart healthy; liver much enlarged and hardened; covered with granulations, like grains of sand; pancreas and spleen enlarged; kidneys healthy.

CASE VIII.—Man, aged 37. Elephantiasis of scrotum two years; death from sloughing. Heart pale and flabby, covered with white deposit; liver enlarged, contained a large abscess; spleen exceedingly pale and shrunk. With reference to the employment of the term "fatty degeneration" in the above, we have to remark that the microscope was evidently employed judiciously, and the characteristics determined by its use.

Dr. Webb observes that, while the Bengalese are especially obnoxious to the complaint, he has met with it in Europeans, East Indians, Armenians, Jews, and Portuguese. Nor does any sex or age afford immunity. Dr. Webb relates the case of a boy, aged one year, who had elephantiasis of the scrotum and penis, and in whom it commenced when he was five months old; of another, aged 14, who suffered under the complaint; and of a girl, aged 17, who had an elephantoid tumour of the right labium, about twice the size of her head.

8. *On Variola, especially with Reference to its Occurrence and Relations in Berlin during the Twenty Years, 1834—1853.* By Dr. QUINCKE. (*Annalen des Charité Krankenhauses, Sechster Jahrgang, Heft 1.*)

IN an analysis of the statistics of small-pox, the author touches upon various points of interest. With reference to re-vaccination, he concludes that it becomes

necessary after the sixteenth year, as he finds that the susceptibility to small-pox increases in persons vaccinated in the first year of life after puberty, in the ratio of advancing age. In confirmation of the correction of this conclusion, he adduces the results obtained in the Russian army. During the years 1834 to 1853, 811,402 military men were re-vaccinated; of these, 457,581 exhibited the genuine vaccine pustule; the total number of those attacked with variolous disease was 421, of which 217 had varicella, 191 varioloid disease, and 13 genuine small-pox. Four deaths from small-pox occurred among the 457,581, who had been re-vaccinated with success; while 25 died of small-pox among the 353,821 who had been re-vaccinated without any result. The reader will gather from what precedes, that Dr. Quinke regards variola, varioloid disease, and varicella as modifications of the same disease, determined by the constitution of the individual, the genius epidemicus, and other circumstances extrinsic to the disease itself. He states that there is no anatomical difference between the eruptions of these varieties, and gives instances which came under his own observation, in which several members of the same family were successively seized with varicella, variola, and varioloid disease about the same time. He regards the various forms of the local affection resulting from more or less perfect vaccination, as evidence in favour of the view of the identity of the three eruptive diseases alluded to.

9. *On the Communication of Syphilis by Vaccine-Lymph.* (Bullet. Génér. de Therap., July 15th and 30th, 1855.)

THE question of the influence of syphilis upon vaccination is one of importance; the opinion is very wide-spread among the laity, that vaccine lymph taken from an unhealthy child generates disease. A case in point has lately occurred in Bamberg, a town of Bavaria, where a medical man was condemned to two years' imprisonment for having vaccinated several children from a child exhibiting a syphilitic eruption on its face and body. The witnesses asserted that the vaccine pustules had not been properly developed, and were followed by tedious ulcerations. Moreover, nine grown-up persons were asserted to be re-infected by the children tainted through the vaccine pustule. The judgment was commuted in consequence of the opinions expressed by Messrs. Heyfelder and Pauli, two distinguished medical men of Rhenish Bavaria, whose judgment has been supported by that of Ricord and Cullerier, who utterly deny the possibility of communication of the syphilitic poison by the agency of vaccine-lymph. Cullerier, according to the 'Bulletin de Thérapeutique,' from which we extract these facts, states that he has not only vaccinated syphilitic children without ever seeing the vaccinia in any way modified by the syphilitic diathesis, but that he has vaccinated healthy children from syphilitic infants without ever perceiving the slightest unpleasant results. The Société de Chirurgie, through their reporter, M. Brocas, have pronounced absolutely in favour of the views of Messrs. Ricord and Cullerier.

10. *Notice of a Tonic Contraction occurring epidemically in Subjects affected with Typhoid Fever.* By Dr. F. A. ARAN, Physician to the Hospital St. Antoine, &c. (L'Union Médicale, July 19th, 1855).

IN the recent epidemic of typhoid fever, Dr. Aran has met with twelve cases in his service since last January, in which, at an advanced stage of the disease, when convalescence was beginning, the contractions showed themselves. With the exception of a single case, in which the appearances of the contraction coincided with the commencement of the disease, the contractions occurred after the sixteenth day of the fever. In none of the patients did the course of the fever appear in any way influenced by the new symptom, whether the former progressed favourably or otherwise. The attacks were sometimes preceded by fornication,

pricking, numbness of the extremities, and pain in the chief joints. The immediate seizure was announced by feelings of great anxiety and distress—it occurred at all times of the day. The contractions affected the upper and lower extremities, and especially the former; the two corresponding extremities were generally seized at once, though the arms presented occasional exceptions from this rule, by being attacked singly. The flexor muscles, which were the seat of the affection, exhibited almost incessant fibrillar contractions. When the entire upper extremity was attacked, the fore-arm was bent upon the upper arm, and lay upon the thorax, while the fingers were doubled up on the palm of the hand; the patient was unable to extend the limb, but by gradual manipulation an artificial extension could be obtained, and this gave the patient relief, though the parts resumed their morbid position when left to themselves. When the lower extremities were attacked, the contractions were less general, the leg was extended, the calves hard and painful, and the toes flexed and bent back. In four cases the muscles of the trunk were affected, producing opisthotonos, during which the patient was unable to execute any movement.

During the seizures, the contracted muscles were the seat of intense pain. The attacks lasted from a quarter of an hour to two or three hours, and recurred from two to ten times daily for several days, more or less. After the cessation of the attacks, the disease ran its ordinary course, without any residuary affection, beyond an occasional numbness of the affected parts. Three of the patients died, but this result was attributable to the severity of the fever, and not to the tetanic affection.

Dr. Aran is at a loss to account for the phenomenon which, according to the inquiries he instituted, appears to have been peculiar to his patients. He was inclined at first to set it down to the mercurial treatment which he habitually employs, but this conclusion is negatived by the fact, that of the twelve patients affected, five took no mercury. The employment of baths was regarded as having benefited the patients, and in one severe case of opisthotonos, chloroform inhalations gave relief.

QUARTERLY REPORT ON SURGERY.

By JOHN CHATTO, Esq., M.R.C.S.E., London.

Communications from the Syphilitic Section and Clinic of the Charité at Berlin.
By Dr. BAERENSPRUNG.—Under this title, Dr. Baerensprung makes several interesting observations upon various points relating to syphilitic diseases, some of which we here extract.

Chancre of the Urethra.—After giving the particulars of the six cases he has met with during the year, he thus sums up:—1. Chancre of the urethra sometimes occurs as the only affection, while, in other cases, chancres co-exist elsewhere. 2. It may be easily overlooked, and then the bubo or secondary symptoms which result may be supposed to be produced independently of primary symptoms. 3. It may lead to perforation of the canal, and consecutive infiltration of urine and urinary abscess. 4. It is most easily produced in persons having wide urethrae. 5. It is most usually situated at the anterior part of the urethra. In only one of the six cases was it as low down as two inches. 6. Stricture may result from its cicatrization. 7. The chancre is always accompanied by discharge from the urethra; but if gonorrhœa does not exist, this is always sparing, notwithstanding the assertions of Vidal and others, who maintain that the contact of syphilitic virus with the mucous membrane of the urethra induces violent gonorrhœa. 8. The presence of a flocculent discharge from the urethra is well calculated to excite suspicion of the existence of the chancre, but does not prove it. In ordinary gonorrhœa, flocculi are not infrequently observed, consisting of emballed epithelial and pus cells. It is only when the microscope exhibits among these

flocculi destroyed corium-fibres, that we can conclude as to the presence of an ulcer. 9. The mixture of blood with the urethral secretions was observed in all the author's cases; but as this may be also present in gonorrhœa, it is not decisive. Bleeding, however, only occurs in gonorrhœa when there is severe inflammation or painful erections, and then it may be very abundant. Small discharges of blood, that are repeated daily, without any cause, and are intimately mingled with the secretion, should always lead to the suspicion of the existence of a chancre. 10. Induration in the course of the urethra is no certain sign; for while it is only found in the indurated form of chancre, we may sometimes, in simple gonorrhœa, feel hard knotty points, due to local exudations into the corpus cavernosum. 11. Oedematous tumefaction of the lips of the orifice of the urethra, which existed in all the author's cases, also occurs in the inflammatory stage of gonorrhœa. In urethral chancre, however, it continues longer, and even when no inflammatory symptoms exist. It is therefore a sign well calculated, in conjunction with the other symptoms, to excite our attention. 12. The positive result of an inoculation affords a certain proof of the existence of urethral chancre; but a negative result is no proof of its absence. In one of the author's cases, in which no result followed inoculation, an autopsy exhibited a chancre; and in another, in which the diagnosis had been derived from the microscope, constitutional syphilis occurred some time after to confirm it.

Dr. Baerensprung indeed believes that the microscope suffices for the establishment of diagnosis. It may be objected, that gonorrhœal ulcers may determine a destruction of the mucous membrane also. This, from analogy and direct observation, he denies. In all cases in which he has submitted gonorrhœal discharge to the microscope, he has only found pus and epithelial cells, and never connective or elastic tissue; while daily experience teaches us, that although blennorrhœa very frequently induces superficial erosions of mucous membranes, as in the prepuce and vagina, it never gives rise to deeper ulceration penetrating the corion.

Abscess of Bartholin's Glands.—This is one of the varieties of abscess of the labia so often met with in prostitutes. Gonorrhœal inflammation is propagated along the excretory duct of the gland, which, in place of its normal, scanty, clear, and filiform mucus, now secretes a considerable quantity of purulent fluid, its vesicles having become dilated, and the entire gland converted into a cyst secreting pus. In some cases, we may press out through the duct as much as a watch-glassful of thin filiform fluid, which, besides pus-corpuscles, contains pavement epithelium. Huguier terms this parenchymatous abscess, distinguishing it from multilocular, which has its site in the interlobular cellular tissue. The latter is of much more frequent occurrence, and rapid development, and may attain the size of an egg. It cannot be emptied by its duct; and if not opened, it breaks, and discharges a thicker pus, with which mortified tissue is mingled. After it has been emptied, the hard and enlarged gland may be felt at the bottom, and if laid bare by further incision of the walls of the abscess, appears as a blood-red granular body. Not infrequently short, wide-mouthed fistulae remain; and in other cases, the glandular texture seems to be destroyed, inasmuch as acinose structure comes away with the mortified tissue. In one case, the place of the gland was entirely occupied by cicatricial tissue.

Among several hundred cases of blennorrhœa in women, the author has met with this affection 47 times in the year. In 30 of these, the blennorrhœa was confined to the duct, while in 17 the gland had suppurated. Although, as a general rule, the blennorrhœa of Bartholin's gland ceases at the same time as that of the vagina, in other cases it is very obstinate, and continues long after the other parts of the mucous membrane have resumed their normal secretions. Such cases require especial attention, for the blennorrhœa here will infect just as readily as that of the vagina. Solution of nitrate of silver, introduced into the duct by Anel's syringe, or touching the orifice with it in substance, hastens the cure. If an abscess forms, it must be opened, and its cavity touched with the nitrate. The same

means usually suffices for the healing the funnel-shaped fistula that sometimes remains.

The author has several times observed condylomata developed exactly in the orifice of the duct, and this is also a frequent site of chancre. It has happened, also, that the inner wall of a glandular abscess has become converted by chancrous poisoning into a large open sore.

Syphilitic Affections of the Rectum: (1) *Abscess.*—A much more serious affection than the abscess of the vulva above mentioned, are abscesses in the vicinity of the anus, between the vagina and rectum, on account of their tendency to produce perforation on one or both sides, and thus give rise to fistulae. The existence of such in phthisical and hæmorrhoidal subjects is well known; but too little attention has been paid to the especial frequency of their occurrence in syphilitic women. In the course of a year, Dr. Baerensprung has met with 16 cases either of such abscesses or the resulting fistulae. The cause has always been a syphilitic ulcer, which in 7 of the cases was seated at the posterior commissure, and in most assumed a phagadænic character. The author at first believed the starting point of the ulcer was a lymphatic gland; but he can discover no lymphatic glands between the rectum and vagina, but only connective tissue abundantly supplied with veins, and on that account especially disposed to inflammation. Sometimes these abscesses acquire a large size, and are extensively diffused around the anus. The explanation of the difficulty of healing such abscesses is usually sought for in the contractile action of the perineal muscles, and especially of the sphincter ani. The author has tried the effect of a subcutaneous section of the sphincter; but, although the action of the sphincter was completely destroyed immediately after the operation, it was recovered again in a few days. A repetition of the section in nowise facilitated the closure of the fistula. Another probable reason for the difficulty of healing such abscesses may be found in the constant transudation of the intestinal gases that takes place, communicating to the pus the fecal odour, even when no communication with the rectum exists. Moreover, the pus is always mingled with a large quantity of coagulated blood proceeding from the abundant veins of the connective tissue. This was observed even in two cases which reached only a small size, and were opened early. The most important rule in treating them is to open them early, in order to prevent their bursting into the rectum. Sometimes, however, they are found so high up, that this can be done only with difficulty, if at all.

(2) *Fistulae.*—From abscesses of this kind originate fistulae. Of these 12 have occurred in the same space of time—3 incomplete, 5 complete, and 4 recto-vaginal. In the first of these, very careful and repeated examination failed to detect any internal opening. Ribes states that he never failed in 75 cases to find this opening immediately above the outer sphincter, and always within six lines of the anus; but the author's experience, both in syphilitic and non-syphilitic cases, leads him to conclude that fistulae occurring after abscess are at first incomplete, and only afterwards open into the rectum; while, moreover, the inner opening, when it does exist, is often higher—viz., in the vicinity of the internal sphincter.

(3) *Stricture.*—Stricture of the rectum often occurs in syphilitic patients, especially female ones; but they usually only come under notice after having long existed. In two cases given by the author, the formation of circular strictures had been preceded by fistulae. In one of these, after performing incision without avail, he accomplished dilatation with bougies; and in the other, after forcing the stricture with the fore-finger, he also succeeded in dilating it by the long-continued use of bougies. In these cases, the altered mucous membrane was alone implicated, the stricture evidently originating in cicatrization of ulcers of that membrane. In other cases, however, the tissues beneath the mucous membrane are concerned, and a larger portion of the walls of the intestine may become thickened and knobbed; such cases often admitting of only palliative treatment. Most practitioners believe that these syphilitic strictures are the sequence of primary affections of the rectum, due to unnatural copulation; and the author has met

with such cases; but he believes they may also be produced by constitutional syphilis, leading to the production of condylomata and ulcerations of the mucous membrane.—*Annalen des Charité-Krankenhauses*, Jahr. vi. pp. 1—56.

On the Utility of Decoction of Rhatany in Keratitis. By Dr. A. QUADRI.—Dr. Quadri observes, that of all the inflammations of the eye, keratitis is one of the most frequent and most obstinate. Experience has proscribed the employment of mineral astringents. Among those of the vegetable kingdom the laudanum formed by the combination of crocus and opium sometimes produces excellent effects; but in serofulous ophthalmia, which is frequently but a keratitis, it occasionally gives rise to prolonged and mischievous irritation. The author had tried various other substances, as tannin, calumba, &c., without any definite results, when he resorted to rhatany. The experience of six years has convinced him of its value. Its application merely induces a sensation of dryness in the interior of the eye, and in a short time the pain and photophobia are mitigated, and the weeping is much diminished. When the irritation has thus become calmed in two or three days, the rhatany may be replaced by the more powerful laudanum, more or less diluted. The rhatany is insufficient in the corneitis accompanying blebnoorrhoeal ophthalmia, but in serofulous and all other forms of keratitis its efficacy is constant. It is prepared by boiling half an ounce of the root in twelve ounces of water, or decoction of elder-flowers, down to half the quantity, and filtering. It should be freshly prepared, and may be used three or four times a day.—*Annales d'Oculistique*, tome xxxiii. 87.

On Wounds of the Heart. By Dr. PURPLE.—No opinion is more generally prevalent than that wounds of the heart are necessarily, and in most cases immediately, fatal. In order to test its correctness, Dr. Purple has tabulated the particulars of forty-two published cases. Among the observations made upon these are the following:

1. Wounds of the heart are, in general, not immediately fatal. The fact of the continuance of the functions of the organ, in spite of severe wounds and the presence of foreign bodies, is sometimes astounding. In a case recorded by Dr. Randall, three shots were found in the right ventricle, and two in the right auricle, the wounds having completely cicatrized. The boy (aged 15) lived sixty-seven days, and died apparently of pneumonia. In a case related by Dr. Constantin, a lunatic lived four days after severely wounding the right auricle and ventricle, transfixing the organ with an ebony paper-scraper. Two remarkable cases are referred to, in which foreign bodies had been long lodged in the heart, no wound of the viscus being discoverable. A soldier died ten weeks after receiving a wound near the left axilla. The ball was found firmly bound down by the *columnæ carneæ* of the left ventricle. No cicatrix was discoverable, and it was supposed that the ball had perforated the pulmonary vein, and passed thence into the left auricle. In the other case, a piece of stick, three inches and a half long, and half an inch in diameter, was found in the cavity of the right ventricle. The patient lived thirty-seven days, and it is not known how the foreign body found its way into the cavity. Another curious circumstance in these cases is observed when the ball penetrates the thorax, and perforates one or both ventricles, without producing any corresponding wound of the pericardium.

2. The cause of delay of the fatal termination is a question of deep interest to both the surgeon and the medical jurist. Besides the circumstances which must obviously modify the result, such as the size, direction, and complications of the wound, the condition of the patient's health, and the treatment he is subjected to, Dr. Purple adverts to another. "The study of these cases has led us to believe that the concussion or shock of the organ has more to do with sudden death than any other one cause. A careful examination of all cases of injury of the heart shows that the primary symptoms are combined in the presence of shock; the suspension of the movements of the organ, the cold, clammy sweat, the death-like

pallor, syncope, and suspension of consciousness, all spring directly from the shock, and are present in a large majority of gun-shot, incised, and the larger punctured wounds. In those cases in which the patient survives the shock, and reaction supervenes, death occurs from other causes, being mainly due to the occurrence of inflammation in some one or other of its forms, consequent upon the injury done to the pericardium or lungs."

3. In respect to *prognosis* the author agrees with Mr. Baird, that these wounds, although always dangerous, are not all necessarily mortal, and that in some cases hopes of recovery may be entertained, especially if the wound has not penetrated deeply, or if the symptoms of consecutive inflammation are of a moderate character. Instances are recorded where the patient has lived a number of years after an incised wound, or even with a ball imbedded in the ventricle. The comparative mortality of heart-wounds shows that the average duration of life is greater if the left ventricle be the seat of injury; the right ventricle being, however, the part most frequently injured.

Dr. Purple concludes his paper with some observations upon the forensic relations of the subject, showing that a wound of the heart cannot be considered to have been necessarily fatal, when the patient has died from the inefficient treatment of the consecutive complications.—*New York Journal of Medicine*, vol. xiv. pp. 411-34.

On Extirpation of Fistula in Ano. By M. RICHARD.—In this communication M. Richard has in view only simple cases of fistula, which constitute four-fifths of those met with. Whether an abscess has preceded or not—and its absence is far more common than usually supposed—a fistulous track extends from some part of the circumference of the anus into the rectum, just above the sphincter. The track is nearly straight, without any diverticulum, is usually very short, and easy of examination, providing this be done more gently than is often the case. The symptoms are much the same in all. There is little or no pain in defæcation, unless there has been great constipation, accompanied by painful spasm of the sphincter; and then, as in true fissure of the anus, it is only some minutes after the passage of the stools that the pain is felt. But there is a constant irritation, which is increased by walking, watching, long sitting, and stimulating food. With this is joined itching, from eczema of the orifice. Baths, rest, regimen, touching the anus and the track with concentrated solution of nitrate of silver or tincture of iodine, assuage or remove these annoying symptoms. The linen is stained by two very different fluids—one colourless, sticky, and staining like semen-spots, is furnished by the eczema or intertrigo; and the other, consisting of prominent, scaly, yellow spots, is true pus coming from the fistula itself. The patient need not be asked whether the linen is stained with fecal matters, as in these cases such never traverse the fistula; but we may learn from him that wind escapes involuntarily from the fundament.

Slight as the suffering from this infirmity may seem, an operation should always be advised; for the reported cases of cure are of doubtful authenticity. Iodine and other injections do not succeed in these simple cases, it being a fact which experience alone could have taught, that multiple, long, and sinuous fistulae are those in which these prove efficacious. The operation M. Richard prefers is that of *extirpation of the track of the fistula*. He has had recourse to it about thirty times, and has always found it innocuous and effective. A grooved director is introduced into the track, the length of which varies from one to two centimetres, and its end is brought out at the anus. This is done very gently, in order to avoid causing pain or laceration of the track, which would prevent the success of the operation. One assistant draws up the buttock forcibly, and another keeps the integuments around the anus as much on the stretch as possible. When the walls of the track are firm, and the folds of the anus well effaced, we may shave along the lower part of the director at a single rapid stroke; but this is a doubtful procedure, for the bistoury, having its back applied to the anal lip of the

sound side, and its edge pressing the internal orifice of the track, may find the latter so soft and depressible as to recede without being cut, and the tissues becoming lacerated during the effort, only an incomplete extirpation results. It is better to plunge the point of the bistoury under the probe to the middle of the track, and having thus fixed the tissues, detach the inner half of the fistulous canal by two or three rapid but well-combined movements, so as to sacrifice as little as possible. Then the external half may be extirpated at one single stroke. Sometimes, when the parts are not kept tense enough, a tenaculum is to be plunged under the track, and the extirpation is practised at leisure by cutting from without inwards. In short, the surgeon must extirpate the canal with as little loss as possible, consistent with the complete removal of the pyogenic membrane.

It may be asked, what are the advantages of this operation, which is more difficult to perform and to bear, and leaves a larger wound than that of incision? The advantages are summed up in a word. We are enabled to dispense with tents and all dressings. Tepid fomentations night and morning, and attention to cleanliness, are all that are required. Two hours after the operation all pain has ceased, and confinement to bed continues only for from twenty-four to forty-eight hours. No accidents or ill consequences result, the wound, in contact with the intestine at one end, and closed in by the margin of the anus at the other, seeming to enjoy all the immunity of subcutaneous wounds. Complete cicatrization may, however, be delayed for even several months, without either patient or surgeon having cause for discouragement.—*Bulletin de Thérapeutique*, tome xlvi. pp. 537-61.

On the Employment of Hæmospasia in Affections of the Eye. By M. WARLDMONT.¹—During a recent visit to England, for the purpose of witnessing the present condition of ophthalmic surgery here, and his report upon which is for the most part very favourable, M. WarlDMont observes he found no use made of hæmospasia in combating the congestion so frequently witnessed after the operation for cataract. In Belgium, this powerful revulsive means was introduced into ophthalmic practice by M. Stiévenart, of Mons, and is now constantly employed in his eye-hospital. Whenever a person who has been operated upon manifests the least tendency to reaction, Junod's exhausting-boot is immediately applied. It is renewed two or three times daily, and sometimes oftener, frequently inducing syncope, and avoiding thus the loss of blood, which it is often of importance to prevent, especially in the aged. M. WarlDMont, who is in the daily habit of employing this apparatus, also strongly recommends it. Its application is very simple, and its employment most valuable, not only under this particular circumstance, but under a great number of others, in which powerful revulsion is indicated, as, for example, in congestive anaurosis.—*Annales d'Oculistique*, tome xxxiv. p. 12.

On Subcutaneous Wounds. By M. BOUVIER.—M. Bouvier, after reviewing the condition of our knowledge with respect to subcutaneous wounds, lays down the following propositions: 1. Subcutaneous, like all other descriptions of wounds, are the source of traumatic irritation, and induce a local inflammation, with which is connected the mechanism of their cure. 2. They are especially characterized by the slight degree of inflammation and irritation they occasion, and the immediate cicatrization, unpreceded by suppuration, which is the consequence. Hence the advantages which different surgeons have, since 1816, recognised as attaching to subcutaneous operations, and in particular those having for their object the section of tendons or muscles. 3. The occlusion of subcutaneous wounds, completed in their second phasis, or after the healing of the puncture in the integuments, gives rise to their relative innocuity and mode of cicatrization, by applying the tissues one against the other, excluding the contact of all foreign bodies, and multiplying the relations of the organizable liquid or blastema with organized and living matter. 4. Suppurative inflammation is excited exceptionally in subcutaneous wounds, in consequence of accidental local irritation, too considerable

an effusion of blood, or defective occlusion of the wound, which then falls into the class of traumatic lesions having communication with the exterior. 5. The cicatrices of the various tissues in subcutaneous wounds healed by immediate organization, do not differ in their nature from those which succeed to many suppurating wounds. The formation of pus is only detrimental to the production of perfect cicatrices under certain conditions.

In respect to this last position, we may extract a few of M. Bouvier's remarks:

Among the circumstances that influence the perfectibility of cicatrices—i.e., the reproduction of the injured tissue—are, the kind of wound, the situation of the divided parts, and the mode in which the healing is effected. Thus, in wounds with loss of substance and of large surface, in which the tissues implicated form separate masses that are entirely exposed, the reparation is usually limited to the production of a cutaneous cicatrix, with some rudiments of cicatrix proper to the other tissues. But if the wound, though an exposed one, be extensive rather in depth than in superficialities, the tissues occupying the deeper parts may become united by cicatrices proper to them, independently of the new skin destined to close the entry of the wound; and such union is even possible when the two sides of the divided tissues are not in actual contact, providing the separation be not too considerable. The distance within which this is possible, and the length of cicatrices that can be thus formed, vary in different tissues.

Suppuration of even deep wounds exerts two kinds of influence on the reparation of tissues other than cutaneous: 1. If the suppuration be so abundant that the extremities of the divided tissues float in pus, and the neighbouring tissues are destroyed around them, consecutive union is very imperfect, or does not take place at all. 2. Even in the absence of such unfavourable conditions, intense, extensive, or prolonged suppurative inflammation may impair the regularity of the reparative action by accidental adhesions, and by the fusion and induration of the different tissues, which it determines far more easily than does the moderate and circumscribed inflammation of subcutaneous cicatrizations. Still this is only exceptional and accidental in the healing of deep-seated wounds. Far more numerous are the cases in which, in spite of the suppuration, the vacancy in each tissue is definitively filled up by cicatricial tissue similar to that which is supplied in non-suppurating wounds. That this is the case with osseous tissue is familiarly known; and the facts in relation to tendinous tissue, though less known, are no less real, as the writings of Ammon, Blandin, and M. Bouvier himself, amply testify. Under the above-mentioned conditions, also, the nature of the substance intervening between divided muscles is the same, whether there be suppurative inflammation or not. The subject of the reunion of nervous tissue is still a matter of dispute: but it has not been demonstrated that the absence of suppuration, *ceteris paribus*, renders the union more complete, or is the sole condition necessary for reproduction.—*Archives Générales*, tome vi. pp. 54—72.

On the Operation for Partial Staphyloma of the Cornea. By Dr. A. QUADRI.—Little has been written upon partial staphyloma, and in this paper the author recalls attention to a mode of operation long since instituted by his father, J. B. Quadri, of Naples. The case in which he exemplifies it occurred in a child, aged 6 years, who, as the result of a serofulous ophthalmia, exhibited a staphyloma at the lower and inner parts of the left cornea, which gradually increased until the pupil was covered; and vision lost, about a third of the cornea remaining uncovered. The patient being etherized, a knife was passed through the cornea, as in the operation for cataract. It was found to be so thin that, after the incision, it became flaccid, and the staphyloma disappeared. The flap was seized by the forceps, and a small portion of its lower part cut off. The eye was kept closed until the third day, when, on examination, a loss of substance was observed at the base of the staphyloma, having a circular form, like an ulcer of the cornea, and being surrounded by a whitish aureola. To prevent prolapse, the eyelids were again care-

fully closed, and the case left to nature. At the end of eighteen days the eye was reopened, and cicatrization was found to be complete. The cornea seemed, perhaps, a little flattened, as compared with that of the other eye. The upper part, which had continued transparent, had descended to opposite the pupil, so that the patient now began to see again. After employing laudanum for some time, the cornea became clearer around the cicatrix, and the portion of it covering the pupil was quite transparent. There only remained an anterior synechia, which prevented the pupillary motion at the lower part.

This observation not only shows the great utility of the operation, but also, contrary to the opinion of Scarpa, the little thickness of the cornea in some of the staphylomas of children. In the present case it was thinner than in the normal state, and had neither the hardness nor alveolar structure indicated by Scarpa.

M. Warlomont states that, since he has received M. Quadri's communication, he has practised this operation for partial staphyloma twice, with success. In the first of these cases, in which there existed a considerable staphyloma at the lower part of the cornea, with preservation of some transparency in the upper segment, the ablation of a flap restored to the cornea nearly its normal form and convexity; and the operation in the second case was still more successful. It is to be observed that the loss of substance practised on the cornea should be much less considerable than would seem to be required for the correction of the deformity. The facility with which the lips of the wound unite renders the operation, when skilfully performed, almost inoffensive; still it is a delicate operation, and for its due performance, especially in children, the induction of anæsthesia is indispensable. As to the place of election for the excision of the flap, we have always to fear, when it is made below, that the resulting cicatrix may intercept some of the luminous rays. When made at the upper part, a portion of the iris might chance to become engaged in the wound, and the pupil might then be drawn up beneath the upper lid. Seeing, too, the difficulty of operating upon the inner side, M. Warlomont recommends that the excision should be made externally, as any cicatrix or traction of the pupil that might result would prove there of comparatively little consequence.—*Annales d'Oculistique*, tome xxxiv. p. 17.

On the Treatment of Artificial Anus with Irreducible Eversion. By M. GOSSELIN.—A man, aged 68, of good health and robust constitution, having undergone the operation for inguinal hernia, October 19th, the intestine was found gangrened, and was excised. Between then and the 20th November the gangrened parts had come away, and an artificial anus had formed, with an irreducible eversion of the intestine, the size of a walnut. No stool by the natural passages had occurred since the operation. The upper part of the small intestine was the part implicated, inasmuch as, half an hour after food had been given, there was an escape of a greenish fluid, without fæcal odour, which continued to flow for some hours. There was no tendency to a spontaneous cure, as manifested by the formation of Scarpa's infundibulum; and the patient was quite unable to bear the application of compresses, which was several times tried. Rapid emaciation ensued, not only on account of the short track the aliments had to traverse, but from the small quantity of these taken, the patient having little appetite, and wishing to avoid the irritation of the erythematous skin produced by the issue of the fluids. He would, indeed, have sunk, had not his strength been somewhat maintained by injecting, two or three times a day, about a pint of broth into the lower end of the artificial opening. Finding however that, in spite of this aid, the patient's strength was still sinking, M. Gosselin resolved to have recourse to an operation for the radical cure of the affection by section of the septum, for which he employed Dupuytren's enterotome. This was applied on the 20th, and removed on the 29th November, the first natural stool taking place on the 27th, stools continuing to pass from time to time. They then became more rare, and the fluids escaped from the wound pretty much the same as before the operation. The eversion, too, increased daily in size, and became more and more irreducible. On examination with the finger,

the septum could now only be felt at seven or eight centimetres' depth, while the two ends of the intestine communicated by a sort of cavity in common. The external opening, though much narrowed, still admitted the thumb, and the integuments were drawn towards it, and had a tendency to sink in there. The obstacle to a definitive occlusion was presented by the irreducible eversion; and M. Gosselin resolved (28th December) to complete the treatment by a supplementary autoplasmic operation. By this he was desirous of procuring a raw surface of the everted intestine, and bring the integuments in contact with it. The mucous membrane was first carefully dissected off, which implied a very tedious procedure, as the thin fragments were easily torn, and it was very important not to penetrate to the peritoneum. Its removal was executed without going beyond the muscular layer, and was rendered easier by the thickening which it and the subjacent cellular tissue had undergone. This gave but little pain, and was attended with little hæmorrhage. The patient had been kept fasting from the day before, in order that the intestinal contents might not interfere with the operation or the subsequent adhesive process. Next a circular incision of the skin was performed, at a centimetre and a half from the edge of the opening, and all the skin between this opening and the incision was removed. This stage was of shorter duration, and only presented some difficulty, by reason of the condensation of the subcutaneous cellular tissue. The denuded surfaces were now inflected towards each other from one side to the other, and retained there by an assistant. The disposition given to them was such that the approximated bleeding parts in reality formed two planes—a superficial one, at the level of which the abdominal wall was in contact with itself, and a deeper one, produced by the contact of this same part with the intestine, deprived of its mucous membrane. Five points of quilled suture were passed through the superficial plane of the wound; and, as the parts were somewhat tense, an incision of six or seven centimetres in length was made on each side of, and at about two fingers' breadth from, the suture—comprising the skin, subcutaneous cellular tissue, and aponeurosis of the great oblique. The patient was kept fasting, and was sustained during this period upon broth gylsters. The sutures came away on the sixth day, adhesion having taken place. Shortly after, however, there was some discharge of intestinal fluids near two of the points of suture, which remained fistulous until the 15th February, by when, through the agency of compression, they had become closed. At the present time, the cicatrix is complete, and feels very solid. At its level, the integuments adhere to a deep-seated, dense mass, which appears to be the everted part quite confounded with them.—*Revue Médico-Chirurgicale*, tome xvii. pp. 334—9.

QUARTERLY REPORT ON MIDWIFERY.

By ROBERT BARNES, M.D. (Lond.)

Physician to the Metropolitan Free Hospital, late Physician-Accoucheur to the Western General Dispensary.

I. MENSTRUATION: PHYSIOLOGY AND PATHOLOGY OF NON-PREGNANT STATE.

1. *On the Modifications of the Uterus at the Menstrual Epoch.* By M. Ch. JUDEE. (Rev. Méd.-Chir., April, 1855.)
2. *Memoir on the Origin and Development of Retro-uterine Hæmatocle.* By M. LAUGIER. (Acad. des Sci., Feb. 26th, 1855. Rev. Méd.-Chir., May, 1855.)
3. *On the Production of Ovarian Cysts from Graafian Follicles.* By ROKITANSKY. (Schmidt's Jahrb. Jahrgang, 1855. No. V.)
4. *On Bleeding Fibroid Tumours of the Uterus.* By J. H. F. ALBERS. (Deutsche Klinik., No. IX., 1855.)
5. *On the Removal of Large Fibrous Tumours from the Uterus.* By MACHER. (Schmidt's Jahrb., No. VI., 1855.)
6. *On the Employment of Alum in the Treatment of Diseases of the Female Genital Organs.* By M. GAUTIER. (Rev. Méd.-Chir., January, 1855.)

1. M. CH. JUDEE relates the appearances found on post-mortem examination of the bodies of three women who died about the epoch of menstruation. His observations are of great interest, and as it is of importance to collect facts of this nature, we give them in detail. CASE 1, was that of a young woman who died of acute delirium, on July 17th, 1853, in the Salpêtrière. A few drops of blood had escaped from the vagina the night before she died. The body of the uterus was voluminous; its walls thickened. On the whole internal surface was seen a layer of gelatinous appearance and consistency, of about two millimetres in thickness. The colour of this was light red; it ceased abruptly at the upper orifice of the neck. The interior of the neck was of a yellowish colour; it appeared to have undergone no modification under the influence of menstruation. The os tincæ was swollen, so as to narrow the orifice; its colour of a deep blue: some spots were black like lung struck with apoplexy. On squeezing it, droplets of blood oozed out. On section (longitudinal) of the organ, the tissue of the body and of the neck was found to be normal; at the level of the lips of the os uteri this appearance vanished, and was replaced by a kind of magma, containing in its interior traces of fibres, presenting much the aspect of pulmonary apoplexy.

Appendages of the Uterus.—The right ligament exhibited marked congestion, and a beautiful capillary network. The same injection was seen on the outside of the Fallopian tube; the inside was in the same condition, especially near the uterine end. The kind of membrane, of light red, described in the uterine cavity, existed also near this point, and was thick enough to nearly close the tube. (Owing, we presume, to a printer's error in the original, we are unable to refer the following appearances to the proper ovary—both are called left.) On the external surface was a prominence the size of a cherry, of a deepish black, surrounded by a red areola, fading away in intensity. On one of its sides was a hole, admitting a probe. On dividing the ovary, there were seen, besides numerous scars, four bodies, of various shapes. The deepest, of the size of a hemp-seed, looked like a white membrane folded many times on itself. The next, somewhat bigger, was principally formed by fibres, crossed in all directions, colour bluish. The third was nearly superficial, bigger than either of the preceding, its colour deep red. The fourth was the biggest of all, and equal to one-third the bulk of the entire organ. It was immediately in apposition with the fibrous envelope of the ovary. It was formed of two distinct parts: one external, of irregular form, toothed throughout its interior, and of a deep red colour; then there was a membrane investing all the toothings, and serving as a pouch, for a clot of a colour much less deep than the most external one. It cannot be doubted that these two latter were corpora lutea.

CASE 2.—A woman entered the Salpêtrière on January 27th, 1854, and died on the 6th of February following; she was seized with delirium, then a few days afterwards the menses appeared, and did not cease to flow till six days before death.

The uterus appeared more voluminous than usual. Its internal surface was lined by a kind of membrane, thick and hard, of a deep brown red. This could be easily raised off by the handle of the scalpel; it disappeared at the level of the neck. The os tincæ was dark red, soft, and easily lacerable. The left Fallopian tube was congested; the left ovary the same, it contained a corpus luteum filled with coagulated blood, of the size of a nut, and on its surface a scar.

CASE 3.—A woman, aged 23, entered the Hôtel-Dieu on August 6th, and died the same day. She had hanged herself.

The uterus seemed increased in volume. The cavity was larger than in the physiological state; it was lined by a soft reddish membrane of five or six millimetres' thickness, easy to remove by scraping with the handle of the scalpel. Examined by a lens, it presented the appearance of a very full and beautiful capillary network, enclosed in a membrane like a mucous membrane. This membrane was continued into the Fallopian tubes, but ceased at the orifice of the os internum uteri. The os tincæ, on the other hand, was strongly congested, and resembled in aspect an apoplectic lung. The right ovary, which was very large, contained a corpus luteum of the size of an almond, showing a scar yet imperfectly closed.

M. Judée enunciates the following conclusions :

1. The blood which escapes by the vagina at the menstrual epoch proceeds neither from the cavity of the uterus nor from that of the cervix.

2. It proceeds entirely from the lips of the cervix, which are strongly congested at that time.

2. The memoir of M. LAUGIER contains a valuable examination of the principal questions relating to the origin and development of retro-uterine hæmorrhage. This name is given to a cyst of blood situated between the rectum and the uterus, which, pushing forward the posterior wall of the vagina, gradually filling the pelvic cavity, and rising above the iliac-fossa, may open by itself in the vagina or rectum; the contents being clots deposited on the walls and in the cavity, together with a thick, viscous, syrupy blood, of the colour of dregs of wine, or chocolate.

The situation, progressive and almost periodical course, the contained liquid, the accompanying metrorrhagia, characterize this cyst with sufficient precision to render the diagnosis almost certain.

The only tumour of the cavity of the pelvis with which it might be confounded, is another sanguineous cyst, that results from tubal or interstitial extra-uterine gestation in those very rare cases in which the rupture of the Fallopian tube and uterine fibres does not immediately, or very quickly, entail the death of the woman. Of this M. Laugier knows but two instances.

The history of retro-uterine hæmatocle is quite new, and as yet incomplete. Is the encysted blood contained in the peritoneum? Can it be accumulated beneath the utero-rectal cul-de-sac outside this membrane? But the most interesting question is as to the origin of the blood.

A specimen has been presented to the Surgical Society by M. Denonvilliers, which suggested the idea that if in spontaneous ovulation the migration of the ovule is imperfect, and hæmorrhage considerable, some blood may fall into the retro-uterine cul-de-sac; and that, if an adhesive peritonitis sequestrate this effusion, the retro-uterine hæmatocle is formed. To look upon the imperfect migration of the ovule as the cause of the hæmatocle must be regarded as a simple hypothesis, and especially because complete injection is not incompatible with retro-uterine effusion of blood; since this sometimes commences some days after menstruation, at an epoch when the ovule is already in the Fallopian tube.

The progressive growth of the hæmatocle also calls for an explanation. But since all observers have remarked the coincidence of a menstrual epoch with the commencement of the hæmatocle, and some go so far as to regard it as the effect of a deviation of the catamenia—another hypothesis less founded than the first—it is natural to assign a part to the phenomena of spontaneous ovulation in the etiology of the disease.

Is there coincidence between the formation of the hæmatocle and spontaneous ovulation? In the first place, the simultaneity of the hæmatocle and the eruption of the catamenia is not perfect. Sometimes the catamenia appear as usual, a metrorrhagia follows, but for two or three weeks, then stops, and on the very next day an acute abdominal pain indicates the commencement of the hæmatocle. In other cases the catamenia have passed eight days, presenting nothing particular, when acute pains arise, a metrorrhagia takes place, and then the hæmatocle. In another case abdominal pains follow a metrorrhagia of three weeks' duration; six months later, the catamenia last five days instead of three, and it is after this more abundant menstruation that abdominal pains reappear. Dating from this moment, there is suppression of the menses, and development of the hæmatocle.

In a fourth case, delay of three weeks; then flooding, lasting for six weeks; at length, hæmatocle.

In a fifth case, delay of two months; then flooding; and eight days afterwards signs of hæmatocle.

In a sixth patient, acute abdominal pains; six weeks afterwards the menses

appear, last for four days, as usual; on ceasing, pains more severe, and growth of the hæmatocele.

In a seventh case, the menses had taken place; then acute and sudden pains occurred; next period, a delay; the menses appear, and last but two days, pains increasing in severity; at the third epoch, menses last but twenty-four hours, and the pains increase at the same time as the hæmatocele becomes developed.

Several facts result from these observations.

1st. Most frequently the hæmatocele begins after the catamenia, sometimes several days after their cessation. Now, according to the theory of Bischoff, the ovule leaves the vesicle at the moment when the menstrual flow ceases. The beginning of the hæmatocele would, on that theory, therefore coincide with the escape of the ovule, or would follow closely upon it.

2nd. In more rare cases the suppression of the menses, or their progressive diminution, is the sign of its appearance.

3rd. An almost constant phenomenon, either after a delay or following a regular epoch, is a prolonged rather than a profuse flooding, which precedes and accompanies the hæmatocele, and is renewed at each epoch of increased development.

There is, therefore, almost complete coincidence between the moment of spontaneous ovulation and the commencement of the hæmatocele.

A second relation between this disease and vesicular ovulation is, that all the circumstances which, during or very near the menstrual epoch, are of a nature to increase the sanguineous congestion of the ovaries, may become predisposing or accidental causes of hæmatocele.

Spontaneous ovulation and retro-uterine hæmatocele have a character in common, which establishes that their seat is in the ovary.

The hypogastric pains felt during menstruation are generally referred to the uterus. A more special examination has enabled M. Laugier to determine, that in the great majority of cases there exists a pain at the level of one or both ovaries. M. Laugier has extended this observation to the origin of hæmatocele; he has observed this unilateral ovarian pain in every case of retro-uterine hæmatocele. In hæmatocele the unilateral pain is much more intense than in spontaneous ovulation. From the commencement the woman has felt it acutely, and at a more advanced period, when the sanguineous cyst occupies almost the whole pelvic cavity, and sometimes rises into the two iliac fossæ, the surgeon may still detect this ovarian pain, which is increased by gentle pressure.

M. Laugier then recites several anatomical observations. In the specimen referred to at the beginning of this article, the ovaries corresponded by one of their aspects to the interior of the sanguineous focus, and both presented on this aspect several apertures opening into the cyst. The interior of these apertures was roughened by reddish fibrinous products. The two ovaries were covered on the peritoneal side with false membranes, which sealed them to the broad ligament.

In a specimen examined in 1853, the only one possessed by the Musée de la Faculté, the ovary opened into the cyst, retains only traces of its tissue. It is an agglomeration of five or six nodules of the size of hump-seed, of fibrinous consistence. They project into the cyst of the hæmatocele. On the peritoneal side, they are covered by the ovarian envelope and false membranes, which make a thick shell.

In another specimen, presented some weeks ago to the Anatomical Society, the left ovary, mis-shapen, globular, and reduced to the size of a nut, formed a part of the wall of the sanguineous cyst, and opened into its cavity. Its tissue was friable, reddish-yellow, and quite similar to the fibrinous *débris* contained in the cyst of the hæmatocele.

Lastly, one of the autopsies related in the thesis of M. Vignès, demonstrates that the ovary may be completely destroyed. All the signs of hæmatocele had been observed during life. At the autopsy, the Fallopian tube and ovary had disappeared.

Here, it is not simply a congestion carried to the point of causing the hæmato-

cele at the time of spontaneous ovulation; it is a kind of breaking-up of the ovary, the tissue of which, profoundly altered, diminishes little by little, and is at last destroyed.

M. Laugier relates a case from veterinary medicine, proving that during the rut in animals, the ovary may be the seat of similar disorders. On the 20th September, 1852, a cow perished rapidly from internal hæmorrhage at the epoch of the rut. The abdominal cavity contained twelve quarts of blood. The left ovary was the size of a man's head. Its cortical envelope, which in the natural state is dense and compact, was so thinned by distension, that its fibres were completely separated from each other. This monstrous ovary was deeply cracked in a line with the greater curvature. The interior presented a tissue giving no resistance to the edge of the knife; it was no more than a reddish pulp, breaking under the finger, and closely resembling the splenic pulp. The corpora lutea were completely isolated from the tissues of this organ; they had preserved their normal character; their more compact structure had probably kept them from sanguineous imbibition. In the interior of the Fallopian tube there was a small vesicle, the size of a kidney-bean, filled with a yellow fluid: this seemed to be the ovule. Who could hesitate in recognising here a hæmorrhage, at first intra-ovarian, and which, if less profuse and less rapid, would have given rise to a hæmatocele?

M. Laugier concludes with the following deductions:

(1.) Spontaneous ovulation is, truly, the exciting cause of intra-uterine hæmatocele.

(2.) The physiological congestion of the ovary during spontaneous ovulation, with persistence of the opening of the Graafian vesicle, does not give rise to hæmatocele.

(3.) For this to be produced, there must be an exaggerated congestion, sometimes brought about by accidental causes, the action of which is exerted during, or some days after, menstruation. Abortions are not, as has been supposed, the immediate causes of hæmatocele.

(4.) It is especially the returns of spontaneous ovulation that gradually augment the volume of the hæmatocele.

(5.) The successive ovarian vesicles open into the hæmatic cyst, and remain gaping, so that the ovary is destroyed by a small number of spontaneous ovulations taking place in the conditions presented by this organ from the commencement of hæmatocele.

(6.) The rupture of a Graafian vesicle furnishing the passage opened for the blood which escapes from the ovary, the cyst of the hæmatocele will be for the most part intra-peritoneal.

(7.) Spontaneous ovulation and hæmatocele have a common character in unilateral abdominal pain, the seat of which is the ovary, in which vesicular ovulation is taking place.

(8.) Rut may cause in animals an ovarian congestion, followed by rupture of the organ—that is, accidents resembling retro-uterine hæmatocele.

3. A woman, aged 26, who had a year previously given birth to a child, and had subsequently menstruated, was supposed to be in the fourth month of pregnancy, when severe and continuous hæmorrhage set in, followed by the discharge of an hydatid mole, weighing three pounds, and death three days later. The left ovary was the size of a child's head, the right of a fist. Both were filled with cysts, varying in size from a cherry to a walnut; most of them contained a coagulum of blood, the surface of which was invested with a layer of pure fibrin. Some of the cysts were filled with a yellowish, others with a greenish, fluid. In one of the cysts, a delicate bladder, consisting of a finely-granulated membrane, was suspended, and was also lined by a membranous delicate layer of fibrin, in which numerous round and fusiform corpuscles were found. Many other cysts contained a coagulum consisting of fibrin, and invested and traversed by whitish membranous matter. At the surface of the ovarian tumours there were, here and there

between the larger cysts, follicles varying in size from that of a hemp-seed to that of a bean, containing a colourless or greenish-brown fluid, with membranous flocculi. All the cysts contained a pulpy, very opaque ovule; their zona pellucida was not well defined externally, and the germinal vesicle was only detected in a single ovule.

4. Dr. ALBERS relates two cases of fibroid uterine tumours, in illustration of the pathological fact, that a rich vascular network is at times observed on those fibroid tumours which project into the uterine cavity, and are invested by the mucous membrane. This erectile network, according to Dr. Albers' observations, is produced only in those cases in which the tumours have reached a certain size—that of an apple at least—and may be the occasion of persistent and fatal hæmorrhage.

CASE 1.—A woman, aged 30, who had borne two children favourably, was received into the Clinique six weeks after her third labour, on account of persistent flooding. On examination, the os was found more open than usual, and bright red blood issuing from it. No treatment availed. The patient died, four weeks after admission, of exhaustion. There was found in the fundus uteri a fibroid growth of the size of a large apple, covered by mucous membrane, half imbedded in the muscular wall, half projecting into the uterine cavity. The mucous membrane was of velvety softness, and consisted of a great crowd of fine vessels, which on one side formed a decided projection; in this place there were open vessels, and the whole spot had a gnawed appearance. This erectile tissue lost itself in the edges in the mucous membrane, but stretched over the whole tumour, which was a simple fibroid. From its size, it was judged that it had existed before the commencement of the third pregnancy.

CASE 2, is described by G. Tuerk, in his Dissertation, 'Iconographia ingentis tumoris fibrosi in utero primæ parturientis. Bonnæ, 1854.' A woman, aged 29, had always menstruated regularly, from the age of 17 until the commencement of pregnancy. Pains came on in the sixth month; it was found that the cavity of the pelvis was almost filled by a soft tumour; the head was high above the brim. After a time the head came down, compressing the tumour, and, being seized with forceps, was delivered. The tumour immediately resumed its former position in the pelvis. The patient did well for a time. But a year later she again miscarried at the sixth month, with great flooding. Two months after this, flooding recurred, and again from time to time at shorter intervals, so that six months after the abortion she was carried off. There was found at the lower part of the right side of the uterus a fibroid tumour, surrounded by muscular fibres; on the left, a smaller globular fibroid, stretching out the mucous membrane, projected into the uterine cavity; and in the walls of the fundus ten more small tumours.

Dr. Albers concludes that the blood poured out of these tumours, comes not from the vessels in the tumour itself, but from the vessels which are found on its periphery. The vascular network that surrounds the fibroid swelling, and causes the flooding, has the greatest similarity with that found in the neighbourhood of hæmorrhoidal tumours.

5. MACHER, of Mainz, has related two cases of fibroid polypi of the uterus, of pathological and therapeutical interest. A woman, aged 46, had for a year suffered from menstrual irregularity, and for eight weeks had observed strong flooding. There was found a fibrous polypus, the size of a child's head. It not being low enough for ligature, a hole was made in a soft place with the finger, and emollient injections introduced. As the polypus some days after was still unfit for operation, several pieces of the fibrous tissue were removed, whereupon a quantity of bloody pus flowed. On the following day the polypus had all vanished, excepting a few membranous shreds, which were hanging out of the os uteri. Nature had effected the greater part of the operation. Under injections—firstly mucilaginous, then of quinine—the remains were removed.

The other case is from Unger:—A large fibrous polypus was cast off spontaneously, removal from the vagina only being called for.

M. GAUTIER reports various modes in which alum is employed with success at the Lourcine, at Paris, in diseases of the genital organs of women. It may be applied in the form of powder to the vagina by means of the speculum, or a piece of cotton-wool of the size of a walnut, containing, wrapped in its centre, a tea-spoonful of powdered alum, and having a thread attached to it, to aid subsequent removal, may be placed in the vagina in contact with the cervix, by aid of the speculum; or plugs of lint, smeared with an alum ointment—half alum, half lard—may be inserted: fine ones may be made for introduction into the cervical canal—these were contrived by M. Guéneau de Mussy; lastly, alum may be used in solution, by injection or irrigation. M. Gautier especially recommends the dry aluminous plug in hsemorrhagia, and in vaginal vegetations.

II. GESTATION: FETAL PHYSIOLOGY AND PATHOLOGY.

1. *Observations on Fœtal Pulsations.* (From Dr. PULLING'S Semi-Annual Report of the New York Lying-in Asylum. Amer. Med. Monthly. March, 1855.)
2. *Reduction of the Size of the Fœtus by Regimen during Gestation.* By M. P. DUBOIS. (Journal de Méd. et de Chir. Prat., and Rev. Méd.-Chir. Juin, 1855.)
3. *On Congenital Dropsy of the Kidneys.* By RUD. VIRCHOW. (Verhandl. der Phys. Méd. Gesellsch. in Würzburg. 1855.)
4. *A Case of Natural Labour in a Negro-girl, aged Thirteen.* By W. S. STOKLEY, M.D. (Med. Exam., Philad. April, 1855.)
5. *A Case of Hæmorrhage during Gestation from Cauliflower Excrescence, and Cure by Excision.* By Dr. JOS SPAETH. (Wochenbl. d. Zeitschr. d. k. k. Gesellsch. d. Aerzte zu Wien. 2nd April, 1855.)

1. IN Dr. PULLING'S 'Report of the New York Lying-in Asylum' are some interesting observations on the fœtal pulsation. The fœtal circulation was examined in 33 cases. In 2, the fœtal heart could not be heard. In the remaining 31, the point at which its pulsations were most distinct, during the last month of pregnancy, was from one to three inches below the horizontal plane of the umbilicus, being situated on the median line once, to its right five times, to its left twenty-five times. In two instances it changed from the right to the left side of the abdomen, a few days previous to labour. The frequency of the fœtal pulse before the commencement of uterine contraction, varied from 104 to 168, averaging 134. In 2 cases, in which the mothers entered the asylum labouring under a severe bronchial affection, attended by much vascular excitement, it was 152 and 160 respectively. The fœtal pulse during labour averaged about 128. Its range was not fully determined, it being subject to great variation. In one instance, in which ergot was given two hours prior to delivery, exciting the uterus to strong contraction, the frequency of the pulsations was immediately diminished from 140 to 128 per minute, remaining thus depressed until the expulsion of the child.

2. A case observed by M. DUBOIS throws some light on the question, whether the size of the fœtus can be reduced by following a particular regimen during gestation? A woman, shortly after pregnancy, suffered intolerable pain after taking food, so that she was afraid to eat; she restricted herself to the most scanty diet until delivery. The child was born at eight months and a half; it weighed scarcely 1500 grammes, but it was very lively, and sucked well. The placenta presented nothing particular but smallness. A case is referred to in which M. Depaul succeeded in reducing the size of the fœtus by spare regimen; and M. Dubois refers to another of a woman who, having escaped after a bad labour from pelvic contraction, was subjected in her second pregnancy to a very strict

regimen, and the child was so small that it was delivered naturally, yet in condition to live.

3. RUD. VIRCHOW'S communication is one of great interest, in a pathological and obstetric point of view:—A woman delivered herself secretly in a wood: the dead child became the subject of medico-legal investigation. The kidneys were found enormously enlarged. The appearances, which are minutely described, lead to the conclusion that the kidneys were affected by cystoid degeneration, with complete atresia of the pelvis and papillæ. Virchow then proceeds to relate other instances of the same degeneration which he had observed. In every case it was found that the point of departure of the cystic formation was ectasia of the urinary canals or of the Malpighian bodies.

Virchow then refers to this affection as a cause of obstruction to labour. Several cases of difficult labour from this cause are known. With such a disease, the prolonged life of the child is scarcely possible.

4. DR. STOKLEY, of Northampton County, Virginia, relates the case of a negro girl, of thirteen years of age, who brought forth a healthy child, of the ordinary size, after a natural labour of a remarkably easy character.

5. DR. JOS. SPAETH relates an interesting case of excision of the os uteri during pregnancy, under the following circumstances:—A woman, aged 29, was pregnant a third time. About the end of the fifth month, without obvious cause, copious hæmorrhage in clots suddenly appeared. This ceased under rest and acids, and at the end of a week a whitish muddy discharge set in. There were no pains. In the eighth month hæmorrhage returned; the fetal heart was heard. A tumour the size of the fist was felt in the vagina, covered with clots, and resembling the placenta in feel. The tumour was attached by a narrow neck to the posterior lip of the os uteri. Blood flowed freely from the surface of the tumour. A ligature was applied round the neck of the tumour, which was then divided by scissors below the ligature. The bleeding ceased, and no fever followed. The ligature fell next day. On the eighth day after excision the membranes burst; the fœtus presented by the breech; it was born alive, but weak, being of eight months' growth, and died shortly. The patient recovered well. Remaining afterwards under the observation of Dr. Spaeth, she suffered no pain, leucorrhœa, nor flooding.

The examination of the tumour showed that it was a specimen of that form of epithelial cancer which Sir Charles Clarke called the cauliflower excrescence.

III. LABOUR.

1. *On Uremic Eclampsia.* By F. WIEGER. (Schmidt's Jahrb. No. 7, 1855.)
2. *Two Cases of Dystochia; one from Enormous Enlargement of the Kidneys, the other from a Carcinomatous Liver.* By MM. SIEBOLD & NOEGGERATH. (Rev. Méd.-Chir., Janv. 1855.)
3. *A Cephalo-Pelviometer.* By ANS. MARTIN. (Schmidt's Jahrb. No. 7, 1855.)
4. *On Turning as a Substitute for Cephalotomy in Contraction of the Pelvis.* By M. DUBREUILH, fils. (Rev. Méd.-Chir., Mars, 1855.)
5. *A Case of Symphysectomy.* By M. MASLJEURAT LAGÉMARD, of Grand-Bourg. (Bull. Gén. de Thérap., tom. xlvii. onzième livrais.)
6. *A Case of Extra-Uterine Gestation cured by Gastrotomy.* By M. ROUSSEAU. (Bull. Gén. de Thérap., Mai, 1855.)
7. *On Extra-Uterine Gestation.* By BREMONT (Gaz. des Hôp. 24, 1855), and MONOD (L'Union Méd. 1855.)
8. *Perineal Fistula left by the Transit of the Infant through the Perinæum.* By J. Y. SIMPSON, M.D. (Edinb. Med. Journ. July, 1855.)

1. DR. WILGER proposes to defend that new view of eclampsia which regards it as a complication of uræmia, against the older views sustained by L'Huillier and Depaul. He divides his memoir into six parts.

(1.) *Critique of the Negative Observations.*—The author seeks to show that the cases of eclampsia without albuminuria recorded by L'Huillier, Depaul, and Mascarel, must be eliminated, either because the albumen was not searched for with sufficient care, or because the absence of albumen does not prove the absence of uræmia.

(2.) *On the Condition of the Kidneys.*—He seeks to invalidate the assertion of Blot that the kidneys in eclampsia are not always diseased, by the collection of as many cases as possible. He maintains that in albuminuria, fibrinous cylinder-casts are always found in the urine, and that these often increase after delivery, and are found, as Braun and Litzmann have shown, so long as there is albumen in the urine.

He gives a table, which shows that up to the tenth day of the puerperal state anatomical lesions in the kidneys are always found; and that profound alterations of the kidneys are more frequent than congestive conditions.

He concludes this section with the following deductions:—*a.* The kidneys may be diseased without albumen being separated in the urine in observable quantity.

β. The absence of albumen at a given time is no sure proof of the absence either of disease of the kidney or of uræmia. The presence of albumen at a given time stands in no relation to the stage of the disease.

γ. The albuminuria increases at the approach of labour, during labour, and the fits of eclampsia.

δ. The kidneys cannot pour out albumen in any considerable quantity, or during a certain time, without becoming clogged up and diseased.

ε. The disappearance of the kidney-disease is often complete and effected in a short time; often it persists in a slight degree, and becomes aggravated in following pregnancies.

ζ. When it persists during the puerperal state, the disease of the kidney induces other attacks, or causes complications, or aggravates existing ones.

η. Albuminuria grows with the occurrence of complications.

(3.) *Albuminuria and Oedema - a. On Albuminuria.*—Is there albuminuria without nephritis? The author cites instances from Simon, Schmidt, Hensch, and Causstatt, to prove the affirmative. As to the curability of the nephritis which attends pregnancy, he shows that as it depends upon transitory conditions, it is not like Bright's disease, which is commonly dependent upon persistent or recurrent external causes, as cold, &c.

By adducing the statements of Blot and Litzmann, which exhibit the presence of albuminuria in fifty-six primiparæ out of a hundred and seventy-eight, and in twenty-two multiparæ out of a hundred and fifty-nine, he confirms the opinion, that the first labour is a predisposing cause of albuminuria and eclampsia.

β. Of Oedema and Anasarca.—The presence or absence of this symptom has no constant value in diagnosis. It is not always present in Bright's disease.

(4.) *On Uræmia and Uræmic Symptoms—α. On Uræmia.*—Uræmia, the result of nephritis, is characterized in its chemical relations by the retention of water and excrementitious matter in the blood, which in its turn is impoverished by the loss of albumen, and sometimes of globules. The excrementitious matter is thus driven to the skin, stomach, or salivary glands, and even to the lungs, or accumulated in the serous cavities or cellular tissue, to be taken up again into the blood, to aggravate the uræmia. For this reason, a strong diuresis may often persist a long time without exercising a remarkable influence on the nervous system. The degree of intoxication can only be determined by simultaneous examination of the blood, and of the urine. He cites two cases of Gegenbauer and Chiari; in the latter, urea and, by the decomposition of this, ammonia, were found in considerable quantity in the blood, and the prophecy that eclampsia would break out was verified. The woman died.

β. On Uræmic Convulsions.—In seventy-eight cases of Bright, Barlow, and Frerichs', there was amaurosis and amblyopia ten times; syncope, nineteen times; ringing in the ears and deafness, ten times; convulsions, fourteen times. The cerebro-spinal symptoms which precede the attacks of eclampsia have the closest resemblance with those of Bright's disease. Out of a hundred and forty cases of eclampsia collected into a table, forty-three showed premonitory symptoms. Of the cases in which the eclampsia broke out before labour, there were forty per cent.; of those in which it began during labour, thirty per cent.; and of those in which it began after labour, twenty per cent., which were attended with premonitory symptoms. As premonitory symptoms the author enumerates vomiting and diarrhoea, but principally headache, disturbance of intellect, and often delirium; cramps and amblyopia, sometimes followed by blindness, not unfrequently precede. The changes of the pulse and pupils are too uncertain to be considered.

As characteristic of the so-called uræmic eclampsia, the author mentions that no predisposition lies at the foundation of eclamptic convulsions; they cannot become habitual; as a rule, the fits are frequently repeated. The disease is never chronic; it makes no periodical relapses, and seldom returns in subsequent pregnancies. The symptoms are those of epilepsy without the cry at the onset.

The question as to the relation between epilepsy and eclampsia the author regards as not settled, but inclines to the view that epilepsy predisposes to eclampsia. Apoplexy of the brain and membranes may cause fatal convulsions, and appears frequently as a complication of Bright's disease, and makes the diagnosis more serious.

(5.) *Etiology and Prognosis.*—*External Predisposing Causes.*—The bad method of living of the poor favours the disease, and is the reason why it is more frequent in lying-in hospitals than in private practice.

Individual Predisposing Causes.—First pregnancies, twin pregnancies, are mentioned. "Dubois has pointed to distortion of the pelvis and rachitis, duration of labour, and mal-position of child, asphyxia, indigestion, mental excitations, as fright, anger.

The mortality is, according to Murphy, 21 per cent.; Blot, 35½ per cent.; Lever, 28 per cent.; the author, 30 per cent. The mortality among the children is given by Blot as 67 per cent., and by the author as 45 per cent. Many children died shortly after birth, without inspection revealing any tangible cause of death. Frerichs, Litzmann, and Braun, ascribe this death to intoxication of the blood.

(6.) *Treatment.*—The author divides the prophylaxis into a remote (against the albuminuria), and a treatment against the uræmic prodromata, shortly before the labour. The peculiar condition of the blood indicates the following fundamental rules of a rational treatment:

a. The blood must be improved by good nourishment, tonics, and iron: Miquel recommends a vegetable diet. *β.* Exciting diaphoresis by baths, &c. *γ.* Gentle purgatives. *δ.* Maintenance of the urinary secretion by gentle diuretics. *ε.* Direct action upon the renal obstruction, by abstraction of blood from the region of the kidneys.

Treatment of the Uræmic Pre-current Symptoms before Labour.—Tartar emetic, vapour baths, and scarification of the œdematous parts, are considered. Chaily recommends chloroform when there is great tenderness in the uterus. The author regards it only as a palliative against the convulsions, and not as against the fundamental evil, the uræmia. *General bleeding* he regards as a precious means. *The expectative method*, the author regards as admissible when the convulsions are not strong, or first appear during the expulsion of the child. *Opium*, much praised by many authors, is, according to the author, chiefly useful after delivery. *Cold affusions* are recommended by Recamier and Booth.

Coma after cessation of the fits, the author treats with diaphoretics, salines, and diuretics, since the condition of the brain is caused, for the most part, not by hyperæmia, but by serous infiltration. Of *revulsive measures*, the author rejects cantharides. Of anti-spasmodics, musk has been useful after too great depletion.

The author then adduces several cases in which artificial delivery was resorted to, showing that often there is a rapid cessation of the attacks after the emptying of the uterus, and that the mortality is about the same.

Abortion.—Convulsions which appear before the period of viability of the fœtus, end for the most part with its expulsion.

In eclampsia before the beginning of labour, he enjoins excitation of the pains and hastening of delivery. He uses *secale cornutum* for this purpose; when the head cannot be reached by the forceps.

2. M. SIEBOLD, of Göttingen, relates a case of dystochia caused by enormous development of the kidneys in the fœtus. The head presented, but the expulsion of the child was arrested by the size of its abdomen. Repeated tractions were required. The circumference of the abdomen was seventeen inches. The child respired a few times and died. On inspection, two enormous tumours were found in the abdomen; these were the kidneys; the two weighed two pounds; each was six inches long, four wide, three thick. They presented convolutions resembling those of the brain. On removing the capsule, small cysts, containing a transparent serosity, were seen scattered about. On section, the cortical substance could not be distinguished from the pyramids; the greater portion consisting of cysts visible to the naked eye. The bladder was empty; the supra-renal capsules were atrophied. By aid of microscope, it was ascertained that the smaller cysts were dilatations of urinary tubuli.

The case related by M. NÖGGERATH occurred in the polyclinicum of M. Kilian, on the 8th September, 1854. The head presented, but was arrested, by failing uterine contraction, in the pelvis. The forceps were applied, but powerful traction, aided by strong efforts on the part of the patient, failed for some time in delivering the head. The head extracted, the shoulders resisted, and were with difficulty delivered. The principal mass of the fœtus, which had been dead ten days, consisted of abdomen, which was four times the normal size. The liver extended from the xiphoid cartilage to the pubis, and stretched across from one iliac spinous process to the other, so as to cover all the abdominal organs. It weighed two pounds and a quarter. The proper tissue of the liver was seen at intervals; but in its greater part it was replaced by a heteromorphous mass, similar to the grey substance of the encephalon. The microscope demonstrated the carcinomatous nature of the liver.

3. M. MARTIN describes a new instrument adapted for the double purpose of measuring the pelvis and the fœtal head. It is more simple than that of Kiwisch and Germann, but requires, as the inventor admits, great practice in order to use it. The instrument is thirteen inches long, of which seven belong to the part to be introduced into the genital organs, and is made of fine steel. It is bowed inwards in such manner that it can embrace the head in every direction, whilst it is bent outwards in a spiral form, in order to take the measures of the pelvis. In the middle part of the instrument are the measure and an index worked by a screw. (These details are insufficient, without plates, to give a clear idea of the construction of the instrument.)

4. M. DUBREUILH, fils, has related to the Medical Society of Bordeaux two cases in which turning was resorted to in contraction of the pelvis in lieu of craniotomy, according to the recommendation of Professor Simpson.

In the first case, a primipara, aged 22, healthy, was in labour at full term. The waters escaped on February 8th, pains having been felt the day before; labour continued throughout the 9th, and on the 10th, M. Dubréuilh being called, ascertained that the head was arrested at the brim; the promontory was greatly projecting, the antero-posterior diameter measuring only eight centimetres. The forceps, which had been tried before, were now tried again, without success.

M. Dubreuilh then resolved upon version; he withdrew the legs, abdomen, and arms without excessive difficulty, but the head resisted powerful traction. The forceps could not be applied. He tried in vain to bring the head through by passing the fingers into the mouth. Smellie's scissors were used to perforate the skull through the mouth. This emptied, the diameter did not lessen, and the neck was felt to yield under traction. The sharp crotchet was then deeply fixed in the right orbit, and at last the head released. It was found to be a very large one, and the diameters very slightly reduced, notwithstanding the escape of the brain. The patient recovered well.

In the second case, a woman, aged 30, was at full term, in her eighth pregnancy. In the two previous labours the fœtus had been mutilated on account of contraction of the antero-posterior diameter of the brim. The same proceeding was about to be instituted, when M. Dubreuilh coming in, proposed version. After unheard-of efforts (*efforts inouis*), M. Dubreuilh brought down the feet, then trunk and shoulders. The head made great resistance; but energetic traction on the body, in the direction of the pelvic axis, brought it forth. The child was dead, and but recently. The woman recovered.

5. M. MASLIEURAT-LAGEMARD relates a case of symphyseotomy, an operation interesting at least on account of its rarity. The subject of it was a woman who had borne two children without anything remarkable occurring. In a third labour—November, 1847—the head was arrested, and symptoms of exhaustion set in: the forceps were tried several times in vain. Turning was then resorted to, the feet were brought down, but the head resisted all endeavours at extraction. The woman had now been three days in labour. Choice lay between Caesarian section and symphyseotomy. The latter was selected. An incision was made in the median line of the symphysis, the cartilage was then divided by a blunt-pointed bistoury. The separation of the iliac bones was effected by pressing with each hand upon the antero-superior spines of the ilia. A slight crackling was heard in the sacro-iliac articulations, and the two branches of the pubis were found sufficiently parted to admit the finger. As soon as this was done, a very gentle traction on the body of the fœtus sufficed to bring forth the head, and finish the labour. The child was dead. A bandage was applied round the pelvis to bring the pubic bones together. Three days after, shivering and acute pains in the right leg set in; phlegmasia dolens became developed. The patient recovered, notwithstanding, and renewed her occupations at the end of fifteen days. M. Maslieurat-Lagémard has seen her recently; she has been delivered again since of a living child, well-formed, without difficulty.

It is right to add that M. Maslieurat states that he was unprovided with the instruments requisite for cephalotripsy or for ex-cerebration.

6. The case of M. ROUSSEAU is one of unusual interest. It was one of primitive abdominal extra-uterine gestation, cured by gastrotomy. A woman who had borne one child was pregnant again. During the first months she felt passing pains in the left abdomen, and during twenty-four hours, vesical tenesmus. The menses were suppressed. Nine months after the presumed date of conception, the movements of the fœtus, which had been felt earlier and stronger than in her former pregnancy, were no longer perceived. Shortly afterwards, a discharge of blood took place by the vagina; and for fifteen days the secretion of milk, which had set in with the cessation of the movements of the fœtus, increased in activity. At this time fever, disorder of general health, and emaciation began, and the patient entered the hospital at Epernay on October 31st, 1852. In November six cauterizations were made on the left iliac region, where the head was felt. On December 6th, the section was completed by the bistoury, and the cavity of the amnion penetrated; the head of the fœtus was opened; the bones of the arch and the brim removed; the placenta and membranes were left *in situ*. The operation was effected without opening the peritoneum. No peritonitis followed; but,

phlebitis in both arms appeared. Emollient injections first, then chlorinated injections, were thrown into the amniotic sac. Quinine was given. The phlebitis ended in recovery. The capacity of the amnios diminished day by day. The placenta continued to live, and took part in the cicatrization, which proceeded rapidly. The wound contracted, leaving only a small fistula.

7. The following cases of extra-uterine gestation are interesting. The first is related by M. BREMONT:—A lady, aged 37, who had borne two children—the last eight years ago—resorted to the thermal baths of Chaudes-Aigues, for the purpose of restoring menstruation, which had ceased, as she supposed, from a cold. Examined by M. Bremont, she was discovered to be pregnant; and it was considered by him to be a right tubal gestation. He endeavoured to hasten delivery by baths, local bleedings, and belladonna to cervix. In some weeks' time, the uterus descended lower into pelvis; and gradually the os externum uteri, then the os internum dilated, until he was enabled to pass the finger into the uterine cavity. He found the orifice of the right Fallopian tube open, like a cut quill, whilst the left was in normal state. Three weeks later, after strong pains, the birth of a dead six months' child took place. For six weeks after, the patient felt pains in the abdomen, and discharge of blood from the uterus, until the escape of a half-decomposed cyst. From that time she recovered.

The other case is recorded by M. MONOD:—A woman, aged 32, had had a natural labour six years before. In August, 1851, she believed herself again pregnant, although menstruation appeared regularly. In October, strong abdominal pains set in, resembling peritonitis, and were met by antiphlogistic treatment. During January and February, 1852, the movements of the fœtus were very painful. In April the patient looked for her delivery, and labour-pains set in, but ceased. Next day, vomiting, great pain in the abdomen, fever, and delirium appeared; antiphlogistics employed. Examination revealed complete anteversion of the womb; by rectum, a large hard tumour was felt behind uterus; external examination discovered a tumour filling the whole abdomen as high as the navel. Extra-uterine gestation diagnosed. Three weeks later the woman was well again. In September, 1854, the swelling had somewhat lessened. Severe pains came on again. A trocar was thrust in on the right of the navel, and about three pints of an odourless, yellow, thick fluid withdrawn; the size of the abdomen having returned, after eight days the paracentesis was repeated, when much pus, mingled with hair, came away. On the 6th January, 1855, five punctures had been made. She was now very pale and emaciated; the abdomen exhibited only doubtful fluctuation, and deep crepitation was felt. The uterus was anteverted; diarrhoea came on, discharging membranous substances and albuminous flocculi. On the 13th January a fresh puncture was made, which gave exit to much stinking pus and discoloured fluid. By means of Vienna paste and an incision, the next day a larger opening was made, through which several fœtal bones were drawn out on the 16th. The patient sunk, and died on the 17th. *Autopsy*:—In the rectum, signs of old inflammation; uterus anteverted; its cavity empty; the os uteri plugged up with a gelatinous mass; left ovary and tube healthy; right tube hypertrophied, pervious. The embryonic sac was attached to the right broad ligament, to the posterior surface of the uterus, to the greater and lesser omentum, to several points of the abdominal wall, and to a part of the rectum; at the end of the sigmoid flexure there was a large opening from the cyst into the rectum, and another led from the cyst into the right Fallopian tube. The walls of the cyst consisted of a double membrane. The right ovarium was atrophied. In the cyst were the remaining parts of the fœtal skeleton. There was no trace of a placenta.

8. Professor SIMPSON describes a preparation in the obstetric museum of the University of Edinburgh, which exhibits a fistula in the centre of the perineum, persisting after perforation by the passage of the child during labour. The Professor also quotes two similar cases, the only ones with which he is acquainted—

the one described by Marter, of Königsberg,* and the other by Halmagrand, of Paris.† M. Halmagrand cured the fistula in his case by dividing the anterior bridle of the perineum, cutting a raw surface at the seat of the fistula, and merely maintaining in apposition the thighs. Reunion and cicatrization took place.

IV. PUERPERAL STATE: LACTATION.

1. *Resorption of the Placenta.* By Dr. SABATIER. (L'Union Médicale, Avril, 1855.)
2. *On Mother's and Nurse's Milk.* By Dr. HERM. PLOSS. (Journ. f. Kinderkrankh. i. 2, 1855.)

1. DR. SABATIER relates three cases in which he thinks the placenta was absorbed.

CASE 1. A woman, aged 35, three and a half months pregnant, aborted of a fœtus after an injury. Copious hæmorrhage followed, but no placenta. The uterus contracted, and the os closed up. On the fifth day shivering and fever set in. Lochia abundant, purulent, and horribly offensive. On the tenth day a violent shivering and expectoration of a great quantity of fœtid matter, having same odour as the lochia. As the expectoration went on, the lochia diminished. The patient recovered well.

CASE 2. A woman pregnant the first time was delivered of two girls, but the placenta did not follow. Dr. Sabatier introduced his hand into the uterus, and felt the placenta partly detached. The detached portion was brought away by the hand. On examining it, there was found a deep fissure corresponding to the adhering part. The patient said that in the sixth month she had received a violent blow from a stick, a kind of crack had been felt in the uterus, and intense pain; the next day a little blood escaped by the vagina. After delivery, shivering, sweats, fever set in on the fourth day, and the lochia began to be offensive. For some days she got worse, until on the twelfth day a fresh shivering fit occurred, pain in the chest, and copious expectoration of fœtid matter. This continued for fifteen days, the lochia having ceased from its commencement. The sweats also ceased from the same time. The patient recovered. No escape of placental tissue was detected.

CASE 3. This patient was only seen by Dr. Sabatier casually. She aborted at the fourth month. The symptoms related were similar to those of the preceding cases.

Dr. Sabatier concludes that it is by the lungs that retained placenta is got rid of.

2. Dr. Ploss's 'Mémorial' is an interesting contribution to a subject in which our knowledge is very defective—namely, the variations that take place in the constituents and nutritive properties of human milk.

The water is increased by improper food, bad digestion, and in a peculiar manner in so-called strong constitutions. The child falls off, becomes anæmic, and its nightly cries indicate an unsatisfied call for nutrition. With this condition there is much urine, and scanty stools. The water is diminished by recurring pregnancy during menstruation, by intervening illness, especially by acute colitis and chronic enteritis. A diminution of solid food, and increased imbibition of water, are recommended; and if pregnancy recur, weaning.

The solid elements are increased under the conditions just named, as in colitis. The milk becomes too nutritive and difficult of digestion. There is diminution of the solid elements when nourishment is bad, in advanced age, typhus, and in chronic tuberculosis without diarrhœa.

Casein appears increased in much-developed breasts, menstruation, acute disease, and mental disturbance. The child soon suffers from constipation, aphthæ, and

* Stebold's Journ. für Geburtskunde, vol. ix. p. 726.

† Démonstrations des Accouchements, p. 577.

lastly, marasmus. The casein is diminished when nourishment is bad, in robust constitutions, chronic diseases, typhus.

The butter is increased in much-developed breasts, pregnancy, acute, and still more in chronic disease. In this case, also, the nutrition of the child is gradually impaired. The woman should take free exercise in the open air, and a diet as free as possible from amylaceous and fatty materials; the child should take the breast more sparingly. The butter is diminished when nourishment is bad, in mental commotions, and tuberculosis with diarrhœa. Here an amylaceous and fatty diet is useful, and bodily and mental quietude.

The sugar is but seldom increased. It is diminished by absolute fasting, in robust constitutions, during menstruation, and in acute diseases. This deficiency may be supplied by administration of milk-sugar to the nursling, whilst the nurse may take amylaceous and saccharine diet.

The salts are increased in acute disease, especially typhus, and these occasion diarrhœa in the child. They are diminished in chronic diseases, especially in those of the intestines. Both nurse and nursling should take phosphate of lime and common salt.

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opinion as to the small relative proportion of the testicular fluid to that secreted by the vesiculae seminales, is correct; and even that the quantity of the former is less than had been imagined. Hence the occurrence of copious ejaculations, even when the absence of spermatozoa indicated that no fluid came from the testes; and hence, also, the liability to obstruction, without pain, from the small *vis à tergo* of the secreted fluid.

Admitting the accuracy of M. Gosselin's facts, we must conclude with him that the following additions to the pathology of gonorrhoeal epididymitis are henceforth necessary:—1. The obstacle to the passage of the semen is generally at the tail or lower part of the epididymis, but may be at some other point. 2. This obstruction is not accompanied by pain. 3. It produces no change in the condition or function of the organs appreciable by the patient. 4. When the obstruction exists on both sides, it must destroy the fecundating power; so, too, if on one side, if the other testicle be otherwise diseased, atrophied, or wanting. In one case of single epididymitis, and varicocele on the opposite side, no spermatozoa were detected. 5. The duration of the obstructed condition may vary. It may be removed any time before eight months, and possibly after a much longer interval.

In the way of treatment, M. Gosselin recommends very active measures, by leeches, and especially purgatives, repeated every three or four days, in the earlier stages of the attack. These are to be followed by mercurial inunctions, and particularly by the free use of iodide of potassium, which he believes, from observation, to possess peculiar properties of resolving chronic inflammatory deposits in the testicle, even those which accompany tubercle or cancer. In gonorrhoeal epididymitis it is very efficacious, especially when given early. The dose to be employed is not to exceed 1 gramme, equal to about 15½ grains.

2. The case here to be considered was under the treatment of Dr. KELBURNE KING, of Hull, and well exemplifies the value of the urethral section in an old, obstinate, impenetrable, and complicated stricture.

The patient (aged 33 at the time of the operation) had suffered from urinary irritation before he was old enough to express his sensations; at school he had always had slow and painful micturition. At 6, or 8 years he was soundled with great difficulty, for stone, but nothing was detected. From that time until his 13th year, small bougies were at intervals passed through the stricture; but after that period no instrument could be introduced into the bladder. Frequent, imperfect, and painful micturition were constant symptoms, but no blood had ever passed. At the age of 24, owing to more serious suffering and threatened retention, an attempt was made to force the stricture, without success, but with the ultimate result of causing perineal abscess and fistula. From the above-named period he became worse, and had continual incontinence of urine, with painful paroxysms of retention. In September, 1851, the patient, being then 32 years of age, came under Dr. King's care. Several fistulae existed on the scrotum, and a hard knot could be felt at the seat of the stricture, midway between the pendulous portion and the bulb. The smallest sized bougie was arrested at four and a half inches from the orifice of the urethra, and the stricture could not be overcome. The urine was muco-purulent, and easily became alkaline. After continued suffering for some months, of a most severe kind, and having a serious effect on his health, another abscess formed in the perineum, and when this was opened, the patient being under the influence of chloroform, an attempt was made to pass a bougie, by the aid of the finger, in the wound. The attempt failed, but a small calculus, about the size of a pea, was removed from the bottom of the abscess. The urine now flowed partly through the wound and partly by the natural passage, and of course considerable relief followed. But no instrument could even now be passed into the bladder; micturition was frequent, and accompanied with much straining. The stream was small, and mostly in drops, much passing by the wound. He had no power of propulsion, and the urine still threw down a copious muco-purulent deposit.

In October, 1852, a small grooved staff was passed as far as possible, getting it fairly into the stricture, an incision being then made through the skin, in front of

the *serotum*, and the induration felt with the finger; the urethra was opened in front of the stricture, and the knife pushed on through the firm substance to the end of the groove, where it struck against a calculus, which was immediately cut down upon, and three or four or five calculi, weighing altogether ten grains, were turned out of a membranous sac. The staff, which had been held in its place, was then easily passed on into the bladder. A No. 6 catheter was afterwards introduced, and retained. The cure followed in the ordinary way: In a month's time only a few drops of urine passed by the wound; No. 9 catheter was easily introduced, and the patient could propel from him a full stream of urine, which he had not done from his early days. His general health became quite re-established. Dr. King informs us that the fistula all healed, and the wound gradually closed. A bougie was passed occasionally.

Dr. King discusses, at some length, the question, whether this was a case of early stricture, subsequently complicated by the calculi? or whether it originated in the formation of a calculus in early infancy or youth, and its subsequent imposition in the urethra? The former view is adopted first, because the symptoms were developed gradually, and not suddenly, and there was no blood in the urine; secondly, because instruments were passed with difficulty up to the age of 12 years, and no calculus had been felt then; thirdly, because the calculi were so small; and fourthly, because there was no occurrence of symptoms, either local or general, at any time, indicating the descent and impaction of the stones. If this view be correct, the case is a very rare one, but the author quotes another, occurring at four years of age, recorded by John Hunter. Certainly the *practical* impermeability of the stricture — an interesting fact — seems to have been due to the thickening and distortion of the canal produced by the local detention of the calculi within it.

The causes of so early a development of stricture are quite unknown.

It is not our intention to follow Dr. King in the discussion of a case of stricture, in which the urethral section was rendered a second time necessary, and which was treated in succession by Mr. Fergusson and Mr. Gay; nor do we here intend to examine minutely the merits of an operation which has undoubtedly been eminently successful.

In reference to the operation itself, it has only to be remarked, that as the grooved staff could not be got through the stricture, the next best course—viz. that of getting it as far as possible—was adopted. We must observe, also, that the section was made through “the front of the *serotum*.”

3. Two cases (xvii. and xxii.) of urethral section are recorded by Dr. DOBIE. The first presents no very remarkable features; in the second case the seat of stricture was only one inch from the orifice of the urethra, and the narrowing was very great. An external incision, upon a grooved direction, was made, and a catheter retained for thirty-six hours. The wound healed on the fourth day.

1. Dr. MUSSEY, of Cincinnati, relates a successfully treated case of recto-vaginal fistula, large enough to admit two fingers. The sphincter having been divided purposely some days before, so as to facilitate the contraction of the fistula, the edges of this latter were pared and brought together by the clamp suture of Dr. Sims, secured by the wires and split shot. The principal feature in the subsequent treatment was the management of the diet, which consisted, for eighteen days, of two, or two and a half *crackers* (biscuits) a day—i.e., under five ounces of solid food. On the eighteenth day a gill of milk was allowed. An elastic catheter, removed and cleaned every second or third day, was kept in the bladder until the eighteenth day. On the seventh day the wires were cut, and the clamp was removed; the wound was united from end to end. On the twenty-fourth day the bowels acted for the first time, from the use of castor oil. More food was then taken, and the patient left the hospital in the fifth week after the operation.

In commenting on this case, Dr. Mussey remarks that he prefers to use a thicker cylinder or clamp than Dr. Sims—viz., one and a half line diameter instead of one line; and to place his wires nearer—viz., one fifth of an inch apart, instead of one third. Moreover, he employs wire twice the thickness of a horse-hair, instead of that of equal thickness.

VI.—AFFECTIONS OF THE RECTUM.

Cause and Treatment of Prolapsus of the Rectum. By M. DUCHAUSSAY.
(Archives Générales de Med., Sept.)

In a short but interesting memoir, M. DUCHAUSSAY reviews the circumstances attending this troublesome complaint, and fixes attention in particular upon the loss of power in the sphincter ani muscle as the chief cause of the descent of the bowel. Moreover, he endeavours to show that Dupuytren's operation, by excising the radiating folds of skin around the anus, and the operation by four touches with the actual cautery, practised by Guersant, act not by causing any subsequent retraction of the cellular tissue, skin, and mucous membrane, but rather by stimulating the sphincter muscle, so that it regains its contractility, and therefore its retentive character. How else, asks M. Duchaussay, do we explain the fact, that the prolapsus is often cured, or does not return after two days, or even after one day, or not at all, after the operation? He points out the fact, that in cases of this disease in infants, three fingers may sometimes be introduced without causing contraction of the sphincter, before the operation by cautery, whilst afterwards, if one be passed, a powerful contraction of the sphincter immediately ensues. As proof that this recovery of contractile power by the sphincter is the cause of cure, a case is mentioned in which M. Guersant had used the cautery too superficially, the sphincter failed to contract, and the disease returned. A second cauterization was followed, on the contrary, by return of the muscular contractility, and the cure was complete.

According to the author, the cautery acts as a stimulant to the paralyzed muscle, just as it will to the deltoid in a like condition. After pointing out the inconveniences and apparent severity of M. Guersant's method, M. Duchaussay suggests that a slighter cautery, or some other stimulant to muscular contractility, might act as well; and he suggests strychnine. This, with M. Guersant's permission, has been tried in the Hôpital des Enfants, in the case of a girl aged eleven years. The prolapsus here arose from obstinate constipation; it had lasted for four years, the bowel protruded at each evacuation about ten centimeters (— 4 inches). During the first month of her admission she was treated by laxatives only, with no other result than that of diminishing the length of the protruded portion of bowel to about four centimeters (1½ inches). Strychnia was then employed endermically near the region of the sphincter; the next day there was no evacuation; on the following day the bowels acted once, only a slight bulging of the rectum taking place; on the third day the protrusion was still less after an ordinary evacuation; and during the next thirteen days, it did not occur again.

Blisters were made in the cleft between the nates, and on the right thigh close to that cleft; one-sixth of a grain of strychnia was applied the first day, one-third on the second, and one-third on the fourth day. On the fifth day, about half a grain of sulphate of strychnia was used, and this was repeated for the last time on the sixth day. In the case of a boy, it is recommended to be applied between the scrotum and anus, immediately over the anterior interlacement of the sphincter ani fibres. The remedy certainly deserves further trial.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. KESTEVEN, M.R.C.S.

MEDICAL Jurisprudence in Great Britain has no distinct organ whereby to vindicate its condition and progress. In Paris, in Berlin, and in Vienna, journals devoted thereto preserve all the most important facts relating to continental forensic medicine. On this side of the Channel these topics must be sought throughout professional journals, or ordinary newspaper reports of trials, coroners' inquests, &c. It is not our object to trace the cause of this blank in British

from that of the upper ranks—Portuguese, natives, and English. Among the last named, I may mention the consul, vice consul, the late Mr. Macaulay, the Hon. Mrs. Macaulay, and Mrs. Pettingall.

Such were the sources from which I obtained that certain information, which proves, beyond all reasonable doubt, that the history of the epidemic at Boa Vista fulfils every condition upon which evidence of the infectiousness of a disease is supposed to rest—viz :

The healthiness of the island before the arrival there of the 'Eclair,' with yellow fever on board.

The outbreak of yellow fever among the inhabitants of the island (where that disease in the memory of man was unknown) speedily afterwards, while the other islands of the Cape de Verde remained unaffected.

The fact, that the first cases were in those persons who were brought into contact with the sick of the 'Eclair.'

The immunity from the disease of distant villages for long periods, until the arrival in them of infected persons, and the introduction of the disease in every district, from infected "foci."

The comparative immunity from the disease obtained by persons who adopted common, but partial precautionary measures against infection.

The absolute immunity from the disease, procured by persons who adopted strict measures of isolation and segregation, which measures must have failed, had the disease depended upon a general atmospherical cause.

According to the reviewer, the epidemic did not break out at Boa Vista until November 20, 1845; while the *Eclair* arrived there in the preceding August. In answer to this gross misstatement, I may adduce evidence to which the reviewer cannot reasonably demur—viz, Dr. King's Report, according to which the different parts of the island were invaded by fever at the following periods:

Fort on Small Island, on Boa Vista, Sept. 16th, or 17th, 1845.

Porto Sal Rey, on the Island of Vista, about October 12th, 1845.

Moradinha, on the Island of Vista, Sept. 14th, 1845.

Rabil—Cabeçada, about October 14th, 1845.

Estacia velha, November 15th.

EASTERN VILLAGES:

Cabeça dos Thurafes. October 27th.

Fundo das Figueiras. October 31st.

João Gallego. November 8st.

In the name of all that is fair and honourable, I ask, how could the reviewer state that the epidemic did not commence until Nov. 20th, when, according to the testimony of Dr. King, as well as of myself, the fever had extended *over even the most distant parts of the island, in all directions, long before that period.*

Are we to believe that such an assertion was made in ignorance, or that it proceeds from a reckless desire to accomplish an end at all hazards, even to the sacrifice of truth?

At page 213 continues the reviewer: "Having succeeded in obtaining from Great Britain a grant of money and supplies of different kinds (in the distribution of which Dr. McWilliam appears to have played the popular part of almoner), in compensation for the losses inflicted upon them by the *Eclair*, the people of Boa Vista were encouraged to repeat an experiment which, on its first trial, had proved so successful, when the sickly season returned, after the departure of Dr. McWilliam in the following year. This gentleman addressed, in Nov., 1846, a letter to Sir William Burnett, announcing the reappearance of the yellow fever at Boa Vista, on the authority of a most respectable and intelligent merchant of the neighbouring island of San Nicolao, who had written to inform Dr. McWilliam that some persons had died, and others were sick of the disease. Dr. McWilliam

suggested that the Director-General might possibly deem the case sufficiently urgent to recommend assistance being sent to Boa Vista, and offered his own services on the occasion, which Sir William Burnett declined, but advised the Admiralty to send Dr. Gilbert King's medical inspector to Boa Vista. On the 23rd December the *Sphynx* man-of-war, with Dr. King on board, anchored at Boa Vista. Two boats approaching from the shore, were ordered to "lie off," and when questioned as to the sanitary condition of the island, the answer was to the effect "that the fever (the second epidemic) had carried off a great number of persons, some of them of respectable station. The disease was raging in Porto Sal Rey and the different villages throughout the island, and some were dying every day." The reviewer next proceeds to state that Dr. King, on landing, found that "besides a case of rheumatic fever," there was "*only one case of endemic fever in Porto Sal Rey*, and that in a few days after he had sufficient reason for believing that "*every other part of the island was equally healthy.*"

Any one unacquainted with the case, upon reading these statements, might be led to suppose that there had been no outbreak of fever at all on the island after my departure, and that, in short, the letter written to me by Mr. George Miller, and the reports afterwards heard by Dr. King at Madeira, were wholly unfounded; nothing more than pure fabrications, devised for the purpose of exciting the sympathy and aid of the British Government, in the form of pecuniary compensation. Here, again, truth must dispel the illusion which these sentences of the reviewer convey. In three days after my departure (July 15th, 1816) fever again broke out in Porto Sal Rey. By Dr. King's own account, not a drop of rain fell until a month afterwards; and the first case occurred in one of the best and cleanest houses of the town (that in which Dr. King himself afterwards resided). The disease soon spread, attacking many persons in Porto Sal Rey; and long before Dr. King's arrival, had proved fatal to eleven persons in that town alone, including some of the principal inhabitants, viz., Mr. Macaulay, Senhor Hypolito Almeida, Senhor Francisco Spencer, Senhor Martines, and others. I have no means of ascertaining the number of attacks and deaths in the villages; but presumptively, they were as great in proportion as at Porto Sal Rey. Were these occurrences, coupled with the horrors already experienced by the people of Boa Vista in 1815, not sufficient to excite alarm? Could no complaint or appeal go forth from them, without their being subjected to the charge of fraud and dissimulation of the most degrading kind?

But in truth, the people of Boa Vista made no complaint except to their own governor-general, and then only for some food to relieve the necessities of the poorer and more distressed of their countrymen. Nor was it *till months after the second outbreak that they received any pecuniary compensation from the British Government*; so that the statement of the reviewer, that as they "had succeeded in obtaining a grant of money from Great Britain," on account of the epidemic of 1815, they were induced to repeat the experiment in 1816, is simply at variance with fact. No survivor among the English at Boa Vista will but feel ashamed that a calumny so cruel and so unfounded should have proceeded from one of his own countrymen against the inhabitants of that island. Had Mr. Macaulay been alive, no voice would have been louder or more indignant than his in refutation of so unjust an accusation.

With respect to the allegation that "John Jamieson exaggerated and misrepresented the state of sickness of the island when the *Sphynx* arrived at Boa Vista," I have to observe, that the man is now in this country, and denies that he gave the description of the then state of things as represented by Dr. King. Jamieson's statement, that "the second outbreak had carried off a great number of persons, some of them of respectable station," cannot be controverted; and although Dr. King, in his recent work, says that he found the island *everywhere healthy a few days after his arrival*, it must not be forgotten that in his Report he talks of treating *fourteen or fifteen cases of fever* during his sojourn there of six weeks.

But supposing we are to grant that Jamieson made the statements which are

